PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. XIV. 1886-87. No. 123.

THE 104TH SESSION.

GENERAL STATUTORY MEETING.

Monday, 22nd November 1886.

The following Council were elected:—

President.

SIR WILLIAM THOMSON, F.R.S.

Vice-Presidents.

A. FORBES IRVINE, Esq. of Drum.
DAVID MILNE HOME, Esq. of Milne-Graden.
JOHN MURRAY, Esq., Ph.D.
Professor Sir DOUGLAS MACLAGAN.
The Hon. Lord MACLAREN.
Rev. Professor FLINT, D.D.

General Secretary—Professor TAIT.

Secretaries to Ordinary Meetings.

Professor Sir W. TURNER, F.R.S.
Professor CRUM BROWN, F.R.S.

Treasurer—ADAM GILLIES SMITH, Esq., C.A.

Curator of Library and Museum—ALEXANDER BUCHAN, Esq., M.A.

Ordinary Members of Council.

Professor CHRYSTAL.
Professor DICKSON.
Professor SHIELD NICHOLSON.
T. B. SPRAGUE, Esq.
Professor BUTCHER.
Professor M'KENDRICK, F.R.S.
THOMAS MUIR, Esq., LL.D.
Professor M'INTOSH, F.R.S.
ROBERT GRAY, Esq.
Dr ARTHUR MITCHELL, C.B.
STAIR AGNEW, Esq., C.B.
R. M. FERGUSON, Esq., Ph.D.

By a Resolution of the Society (19th January 1880), the following Hon. Vice-Presidents, having filled the office of President, are also Members of the Council:—

HIS GRACE THE DUKE OF ARGYLL, K.T., D.C.L.
THE RIGHT HON. LORD MONCREIFF of Tulliebole, LL.D.
THOMAS STEVENSON, Esq., M. Inst. C.E.

Monday, 6th December 1886.

JOHN MURRAY, Esq., Ph.D., Vice-President, in the Chair.

1. Chairman's Opening Address.

It is my privilege to welcome you at the commencement of this new Session, which, from many indications, promises to be one of great activity among the Fellows of the Society.

Since the close of last Session our esteemed President, Mr Thomas Stevenson, has placed his resignation in the hands of the Council, and, although asked to reconsider his decision, he urged, after consultation with his physician, that, on account of his failing health, he could not continue to hold a post, the duties connected with which he was quite unable efficiently to discharge. I feel sure the Fellows will join with me in hoping that Mr Stevenson may soon be blessed with a return of good health, and that he may long continue to be a contributor to the work of the Society.

Our new President, Sir William Thomson, is not a stranger to the office; when he last occupied the Presidential chair he conferred many lasting benefits on the Society, and his re-election augurs well for the future.

The vote recently taken among the Fellows, with reference to the proposal to change the hour of the ordinary meetings of the Society, has resulted in a majority for the usual hour of meeting being retained. Still, as there was a large minority in favour of some change, the Council will most probably arrange to hold some meetings at four o'clock in the afternoon during the present Session.

Whether we look at the membership of the Society, the extent and value of its publications, or the general activity of the members with reference to scientific investigations, we have every reason to congratulate ourselves on its prosperous condition, and to cherish the notion that the Society has entered on the second century of its existence with a vigour and prospect of usefulness unknown even at any previous period of its career. This very prosperity, however, brings with it new duties and responsibilities. Some new matters of vital importance to the welfare of the Society are now forced on the consideration of the Council and Fellows : it is to some of these that I propose to refer this evening.

The membership of the Society, including Foreign and Honorary Fellows, numbers at present 507, which is just about the strength of the Royal Society of London. The number of ordinary Fellows is, however, increasing at a somewhat rapid rate, and it is freely discussed, both among the Fellows and in the Council, whether the time has not arrived when only a limited number of Fellows should be elected each year ;—after the manner of the election to the fellowship of the Royal Society of London and some foreign societies.

At present, when a candidate is proposed by four or more Fellows, the application remains for several weeks under the consideration of the Council, and thereafter, if the Council be of opinion that the candidate is likely to be a useful member of the Society, he is recommended to the Fellows for election. It would be a mistake to suppose that all the names submitted to the Council are, as a matter of course, recommended for election; it not unfrequently happens that names are withdrawn while under the consideration of the Council, and some never pass the Council.

I have no hesitation in saying that I believe it would be a great mistake to depart from this method of election, which has worked so well in the past, and has secured as Fellows of the Society representative men of all social positions, and from every department of human activity and effort.

Why should we seek to limit the membership? Every energetic scientific man, and every man who is able and willing to assist in any way in the discovery of new facts, new principles, new processes, new knowledge, is a new strength to the Society, and should be welcomed. If thirty or forty such men become candidates in each year, let us have them all as Fellows; the day has passed when it is possible to number the elect either in science or literature !

If we were to adopt the system of selecting a definite number from the candidates of each year—this means the placing of these candidates in a sort of competition with each other for the vacancies, a most objectionable thing among grown-up men—we place a very disagreeable duty on the Council ;—canvassing would arise; dissatisfaction would follow, however carefully the selection be made; some men who would be most desirable Fellows would be prevented from ever becoming candidates, and offence would be given to the unsuccessful, whose names would ever afterwards be pilloried in the *Proceedings* of our Society. A less objectionable plan would be for the Council to invite a certain number of representative men to become Fellows each year, but it is doubtful if this would work well so long as fellowship involves the payment of fees. There seems to me nothing to fear from an increase in the number of Fellows from year to year. The larger the membership, the more are we likely to be in sympathy with the rapidly increasing number of the general public who interest themselves in the acquisition of new knowledge. We will be all the more powerful when we approach the Government on matters of public interest, and none the less able to give advice in Scottish scientific affairs. In his anniversary address, a few days ago, to the Royal Society of London, the President suggests that that Society should be strengthened by the election of a number of distinguished literary men. This appears to be an admission that the election of fifteen from among candidates each year does not secure that diversity which it is desirable to have in a representative Society, and furnishes an additional argument against the adoption of such a method of election in our Society.

One of the best evidences of the prosperity of the Society is to be found in the great increase in the size and value of the Society's publications. If we include the extra volumes on the Ben Nevis observations and on the Botany of Socotra, which will shortly be issued to the Fellows, then the *Proceedings* and *Transactions* of the Society during the past three years probably surpass in bulk and importance those of any other Society in the United Kingdom for the same period. This must be gratifying to the Fellows, for the money value of the publications in these years is greater than the sums paid in annual fees. The illustrations for these papers have, it is true, been a great drain on the funds of the Society, but the money has been well spent. Just as it is the function of a Society like this to publish in great detail new observations and discoveries, which it would not pay an ordinary publisher to undertake, so should the illustrations of these papers be kept up to a high standard of artistic merit. It is too often the case that the funds of Scientific Societies demand that the cheapest and not the best shall be undertaken in the matter of illustrations.

I commend it to some of the richer Fellows, if they might not see their way to giving or bequeathing to the Society a fund from which the Council might draw to assist in the careful and artistic execution of such illustrations as may be inserted in the Society's publications.

The magnificent and valuable library of the Society, which Mr Gordon informs me now approaches to twenty thousand volumes, has been acquired chiefly by obtaining the *Transactions* and *Proceedings* of the other learned societies in exchange for our own. Foreign societies have always paid us the compliment of placing a high value on the work done by our Fellows, and published by us. As a consequence we have not only the publications of the great academies which have been long established in the great capitals and other cities of Europe and America, but contributions have flowed into the library from the remotest parts of the world: from Shanghai, Hong-Kong, Japan, and Java in the far east, as well as from our great dependencies and colonies of India, Australia, and New Zealand. Again, we get *Transactions* and *Proceedings* from the far west: from Rio de Janeiro, Buenos Ayres, Bolivia, and Mexico, as well as from many rising cities and towns in the United States and Canada.

At every meeting of the Library Committee fresh proposals of exchange from Societies and Universities, not in scientific communication with us, have to be considered. As education and enlightenment penetrate the various countries of the world, and extend their vital influences to new centres of population, new societies are formed, and, with the same desire for exchange on the part of the new societies daily springing into life and activity, the acquisitions of our library must go on increasing at a rapid rate, not only from old but from new sources. In addition there are the donations from Fellows and others, and the purchases which are made annually. Unfortunately, the space at our disposal is now so limited and inadequate that a considerable part of the library cannot be referred to or consulted. This fact cannot be too widely known, in order that active steps may be taken to provide a remedy.

That remedy is not far to seek; in the new arrangements which must follow the removal of the Antiquarian Museum, the Society's library should be spread over the greater part of this noble building, which could not be devoted to a higher or better purpose than accommodating, for the behoof of the leading scientific society of Scotland, the literature of the learned Societies of all nations,—the accumulated records of the scientific researches of the world.

The Council has considered it expedient to lay these facts regarding the library before the Board of Manufactures, and has requested the favourable consideration of the claims of the Society for more space in this building.

For the prosecution of scientific investigations the *Transactions* and *Proceedings* of learned societies are not only invaluable, but indispensable. They fulfil the twofold purpose of showing what has been accomplished in each department of science, and of serving as starting points, on our part, of new advances into the unknown.

Mere scientific compilations, as distinguished from monographs, however useful to the student, are comparatively of little service to the investigator who is striving to extend the boundaries of his science; and it may be affirmed that many original researches, which have conferred much honour on this Society, could not have been successfully prosecuted without the aid derived from the literature received from similar institutions showing what had most recently been done in the same departments by workers in other parts of the world.

It could be wished that the funds at the disposal of the Society would permit of the acquisition of the records of scientific voyages, which embody valuable original observations and investigations, as well as numerous special monographs. Meanwhile, it is gratifying to know that among the large and varied collection of memoirs existing in this library, there are some which were searched for in vain in the greatest of the metropolitan libraries, although that is annually supported by large Government grants.

Let me give an illustration of the value of this Society and its library to the community.

When some six years ago I succeeded to the direction of the work connected with the publication of the scientific results of the "Challenger" Expedition, there was then no reason why the work should be continued in Edinburgh, and I was on the point of recommending the Government to transfer the office to London, chiefly on account of the difficulty connected with the access to a fully equipped scientific library. Had the recommendation been made it would, without much doubt, have been adopted. It was not made, because I was assured by the Council of this Society that every facility would be given to me, my assistants, and strangers who might be engaged on "Challenger" work, for consultation of books in the Royal Society library, and at the same time the University conferred certain privileges of a somewhat similar kind.

I suppose it is not altogether a matter of indifference to Scotsmen to know that the work connected with the largest scientific publication ever issued by any country or age, has been chiefly carried out in Edinburgh, or that Edinburgh would have liked it to be said that, having once been commenced here, it was impossible to carry it on in this city. But over and above the mere sentimental aspect of the matter, the retention of the "Challenger" Office in Edinburgh has been a distinct material advantage to the country, for the work which has been given to printers, binders, lithographers, artists, wood engravers, and others, represents the expenditure of many thousands of pounds annually. Then, there is the indirect advantage of having many scientific men from abroad coming and carrying on work here for short periods of time, and I am bound to say some of them would have remained much longer had the library facilities been better. Again, some special industries, such as lithography, have in consequence been greatly developed in our midst. When the "Challenger" work was first commenced, it was believed to be impossible to have the finest kind of lithographic and engraving work done in the United Kingdom; some authors even stipulated that their illustrations should be done abroad. But now this lithographic work can be done as well here as anywhere in the world, if not better; and I frequently receive from abroad requests to have this kind of work undertaken by Edinburgh firms. It seems to me, then, that anything that can be done to increase the completeness or accessibility of the Royal Society library is to be regarded, not as a favour conferred upon a small body of savants, but as a direct boon to the city and the country of which it is the metropolis.

A Society like ours has a very special interest in seeing a truly complete national library established in Scotland, for to scientific and literary investigators such a library is the most important instrument of research. We must all of us rejoice that there is to be a Free Library in Edinburgh, and it is to be hoped that it will be established so as to give the public the freest possible access to all the ordinary standard works and current literature. It may even be hoped that the directors of the institution will be able to specialise to some extent in the reference library, so as to embrace works not now to be had in Edinburgh.

It must be remembered, however, that this Free Library will mainly be a duplication of books already in Edinburgh libraries. It in no way solves the question of a National Scottish Library, which is the great desideratum for all engaged in the study of science and literature.

There is a great wealth of libraries in Edinburgh, but they are to a large extent inaccessible from want of room or from the antiquated machinery connected with their proper consultation. There is a popular belief that the Advocates' Library contains every English book. As a matter of fact, it is very defective in the literature of some periods; it lacks many important provincial publications, and is of course very deficient in Indian, Colonial, and American works, which are every year becoming more important. There are relatively few foreign works in the Advocates' Library, and were it not for the excellent series of foreign treatises in the University Library, Edinburgh would be very poorly supplied in this respect.

If I wish to consult all the authorities who have described the ice of the Antarctic regions, I can find only some of the books in Edinburgh. If I wish to consult the original authorities who have described the desert of Sahara, I can find only one or two of them in the Edinburgh libraries. Gaps like these are not confined to scientific or geographical works. Socialism is a subject not in the background at present, yet one will seek in vain among our libraries for some of the works of the best-known continental exponents of the theory.

Every true Scotsman has an interest in seeing these defects speedily remedied.

It is much to be regretted that there is no organisation by which reference to the various libraries in Edinburgh could be facilitated to those engaged in scientific and literary work. I have never received a book from the Advocates', Physicians', and some other libraries without being indebted to the courtesy of some member of these bodies; and although this be always willingly given, it becomes irksome to all parties when repeated week after week. In connection with our "Challenger" work, we often find it more convenient and expeditious to get books from London than from certain of the Edinburgh libraries. It is not for the credit of the city that this should be the case. I cannot but think that it would be a great advantage to form a central board, composed of representatives of the different library-owning societies and corporations of the city. Such a board would do good service by preventing the duplication of purchases among the various libraries where unnecessary. It might have the power of granting the privilege to investigators of consulting all the libraries to a limited extent; but, more important still, it might draw up a scheme for a National Library—a scheme which, while allowing existing libraries to develop on their present lines, would yet erect one of them, say the Advocates', into a National Library, whose function it would be to supplement or fill up in those departments not embraced by the other libraries. It seems to me that some such scheme would command support.

When we remember the large sums of public money that are annually spent on national libraries in London, and that in addition to the cost of buildings and maintenance, about £18,000 has been spent during the past ten years on salaries and purchase of books for the National Library in Dublin, then surely the claims of Scotland deserve some consideration. Were a good workable scheme drawn up by some of our leading men, and supported by the public generally, then even Scottish Parliamentary representatives might awake to the fact that there are some Scottish questions worthy of their attention and combined action.

Some time ago the Council of the Society drew the attention of the Government to the fact, that no bathymetrical survey of the Scottish freshwater lochs existed, except those of Loch Lomond and Loch Awe; and urged the importance, in many branches of scientific inquiry, of knowing the depth and form of such basins as Lochs Morar, Maree, Lochy, Assynt, Linn, Tay, Ericht, with many others, and expressed the hope that these surveys would be undertaken at an early date, and, at all events, before the completion of the Ordnance Survey of the country. The reply from the Treasury was that these surveys could not be sanctioned, because they did not come within the function of the Board of Admiralty or of the survey department of the Office of Works. This matter was subsequently brought up in Parliament by Lord Balfour of Burleigh, but no steps seem to have been taken to carry out the survey. It may be hoped that this matter will not be allowed to drop. Quite recently soundings of 175 and 180 fathoms have been obtained in Loch Morar. This is the greatest depth that has hitherto been found on the plateau on which the British Islands are situated; to get depths equal to this we must go towards the deep gut off the coasts of Norway, or beyond the 100-fathom line off the coasts of Ireland. There are also geological and biological problems of great interest connected with the depths of these lochs.

Should these surveys not be undertaken a very important part of the survey of the United Kingdom will be left untouched; for it cannot be denied that it is at least as important—sometimes much more important—to know the depth of a lake than to know the height of an adjoining mountain. It would be a matter for great regret if the admirable surveys, which are now drawing to a close, and which reflect so much credit on the officers who conducted them, and honour on the scientific reputation of the country generally, should be lowered in value by the great omission here pointed out.

The Council has recently had before it the subject of Antarctic exploration, and has drawn up and printed a number of suggestions as to the investigations which should be undertaken or attempted in the event of such an expedition being fitted out. There can be little doubt that a thorough exploration of these unknown regions would enrich almost every branch of science with valuable observations; the Antarctic appears to exert a controlling influence on the atmospheric and oceanic circulation and magnetism of the globe, and presents many interesting physical, geological, and zoological problems. No steam vessel, protected for ice, has ever penetrated these seas, and there are good reasons for believing that such vessels would be able to find a place for wintering close to the land of the Antarctic continent. We have at present no knowledge of the condition of the Antarctic regions except during the summer months of January, February, and March, and, although the first endeavour to pass a winter in these regions would doubtless be accompanied with considerable risk, still it must be attempted, and the duty of attempting it lies heavier on Great Britain than any other nation. If Great Britain is to hold her proper position among the family of nations she must explore the Antarctic, and it is to be hoped that the numerous learned Societies of the United Kingdom will before long press the matter on the attention of the Government.

During the past few years there has been great activity in the examination of the biological conditions of the coasts, lochs, and estuaries of Scotland, and some of the more important results have appeared, or are about to appear, in the *Transactions* and *Proceedings* of the Society.

In connection with the Scottish Marine Station, carried on under the auspices of the Scottish Meteorological Society, there has been conducted during the past three years a very valuable series of investigations into the physical and chemical conditions of the Firths of Forth and Clyde, and various rivers and estuaries, which are of great importance to a right understanding of the general meteorology of the country.

The little steamer of the Station, fitted with the most approved apparatus, has been constantly at work at all times of the year. Three years' observations on the Forth have given the general conditions with regard to temperature and salinity for all seasons, with the laws of their changes.

About the time of the vernal equinox all the water of the Firth is of a uniform temperature; there is a gradient of temperature from river to sea and from surface to bottom in summer, the warmest water being on the surface and towards the land. In winter this state of matters is entirely reversed, at the autumnal equinox there being again a uniform distribution of temperature.

Nearly a year's observations have been completed on the much more complicated and varied region of the Clyde. Here a vast amount of heat is stored up in the waters of the deep lochs during summer, and slowly given out again to the air during the winter months, thus greatly modifying the climate of the West Coast of

Scotland—a condition of things to which there is apparently nothing analogous on the East Coast.

This influence is due to the great depth of the lochs and their exposure to summer heat, and does not appear to be entirely independent of the effects produced by the warm Atlantic water, which flows over the shallow ridge stretching from Cantyre to the Ayrshire coast. Some of the preliminary results of these researches have been presented to the Society by Dr Mill, but all the observations are now in course of preparation for publication, and will furnish physicists and meteorologists with many much-needed data.

I have already referred to the extra volume of the Society's *Transactions*, containing the Ben Nevis Observatory observations, which will shortly be in the hands of the Fellows. Among meteorologists in all parts of the world there has long been a desire expressed to be furnished with copies of the Ben Nevis observations, and as the Directors have no funds for the purpose, the Council have in the circumstances considered it a duty to undertake their publication.

The Fellows are reminded that the Observatory buildings and the road thereto are the property of the Royal Society of Edinburgh, that several donations from the Society's funds were made towards covering the expenses of the preliminary observations before the erection of the permanent Observatory, and that the direction of the Observatory is largely in the hands of the representatives and Fellows of this Society. The Society has, therefore, a deep interest in all that concerns the welfare of this unique high-class Observatory.

As you are aware, the Observatory was erected in the summer of 1883, and formally opened by Mrs Cameron Campbell of Monzie, the proprietrix of the land, on 17th October; immediately thereafter Mr Omond and two assistants went into residence, and the regular work of hourly observations began in the end of November; the Observatory was equipped with the best instruments that could be obtained, several of these being of a novel character, suited to the peculiar climate of the Observatory and to the new lines of observation it was proposed to carry out.

Ben Nevis was selected not merely as the highest mountain in

Great Britain, and situated in the very track of the south-westerly winds from the Atlantic, which exercise so preponderating an influence on the weather of Europe, but because it rises to a height of 4406 feet so close to the sea that a sea-level station may be placed at about 4 miles from the summit—a consideration which gives a value altogether unique to the observations.

The low-level station was established at Fort William, under the charge of Mr Livingstone, of the Public Schools, at which observations are made five times each day, and with these are conjoined a barograph and thermograph for the continuous record of these important elements of climate—atmospheric pressure and temperature. Three years' observations at this pair of stations have already been made, and it is these which are now in the hands of the printer, and will shortly appear as a volume of the Society's *Transactions*.

The climatic difficulties were great, but they were successfully surmounted (1) by the skill of the architect, Mr Sydney Mitchell, who constructed the buildings, (2) by the heroic endurance and fertility and readiness of resource displayed by Mr Omond and his staff of assistants in meeting emergencies as they arose.

At most, if not all, other observatories only a comparatively few of the observations are made by the observers personally, the usual course being to use continuously recording instruments from which the omitted hours are interpolated. But on Ben Nevis every recorded observation is actually noted by the observers. Further, it usually happens that the observations of the temperature, humidity, and movements of the air, and the rainfall are automatically recorded. But on Ben Nevis, during the larger portion of the year, owing to the snow-drifts and ice-incrustations formed on the instruments as well as on everything outside the Observatory exposed to the wind, these observations cannot, and, it may be confidently predicted, never can, be made by self-recording instruments. Hence, if meteorology and weather prediction are to make advances in those fundamental inquiries, which can be successfully prosecuted only by the help of high-level observatories, the time will never come when such heroic services as are now rendered to science by Mr Omond can be dispensed with. Even barometric observations could not be utilised in these inquiries, unless there be conjoined with them observations of the temperature and the movements of the atmosphere.

The high expectations formed of the great importance of this Observatory have already been more than realised. The following are among the more prominent of the results obtained from this pair of high and low level stations:—

1. The rate of decrease of temperature with height has been more correctly ascertained.

2. The rate of decrease of atmospheric pressure with height for the different sea-level pressures and air-temperatures has been ascertained with a high degree of accuracy.

3. The relations of the readings of the dry and wet bulb hygrometer to the vapour of the atmosphere have been worked out, under the extraordinarily dry states of the air which are of such frequent occurrence on the top of the mountain, from observational data to a degree of accuracy not hitherto attained.

These results will, doubtless, in future be incorporated in all books on meteorology and general physics.

As regards weather forecasting, the Ben Nevis observations contribute information of such a value as no low-level observatory, however efficiently equipped and superintended, can for a moment lay claim to; and it is to be enforced here, that when the Directors are placed in a position to raise the five daily observations at Fort William to twenty-four, the value of the Ben Nevis observing system will be immensely enhanced. In other words, the Directors of the Observatory attach the greatest importance to the establishment of a low-level observatory at Fort William, at which observations could be made with the same fulness as at the Observatory on the top of the mountain.

The great value attached to the observations of high-level observatories is attested by the continued additions made to these stations in different parts of the world, and by the observations made at these stations many of the more important questions of meteorology may doubtless be investigated. But there is no other high-level observatory, and its conjoined low-level station, so happily situated as Ben Nevis Observatory and the station at Fort William for supplying physicists with observational data. This is due to the fact that the former is on a peak and the latter close to the sea, the ground sloping down to it, by which the effects of solar and terrestrial radiation are minimised.

It is only a few months ago that an inquiry, conducted on scientific principles, into the bearing of the observations at the Observatory and at the base of the mountain could be entered upon, and its completion will necessarily occupy some time, owing to the complex character of the problems to be dealt with.

The erection and maintenance of the Observatory during the past three years have cost over £7000, the funds having been obtained solely from private subscriptions and learned Societies. During this time the Directors have had to pay to the post office department a rental of £133 per annum for the use of the telegraph wire from Fort William to the top of the mountain. In addition to this, the post office have received considerably over £100 for tourist messages forwarded from the Observatory by Mr Omond and his assistants. The daily despatch of the observations to the press throughout the country is also a source of considerable income to the post office department. It appears then that this Observatory, established solely for the purposes of scientific investigation, instead of being assisted by the Government, is actually a source of revenue to a Government department.

You are aware that a sum of £15,000 is voted annually by Parliament for general meteorological purposes, but more particularly the meteorology of the British Islands. This grant is administered by a committee nominated by the Royal Society of London, called the Meteorological Council. Although Ben Nevis Observatory is certainly the best and most important meteorological observatory in the United Kingdom, yet it has received no support from this grant, if we except the annual sum of £100 which is paid on condition of being supplied with a complete set of all the observations,—a bare equivalent for the mere clerk work required. In 1884 an application for £300, from the £15,000 grant, towards the expenses of the Observatory was refused.

The Meteorological Council also intimated to the Directors that they did not propose to have the observations at Ben Nevis wired to London for use in making up the weather forecasts, until the result of Mr Buchan's discussion of the observations was completed; but they offered no assistance towards carrying out that discussion, although £1000 of the £15,300 is expressly stated to be for the purposes of original investigation

An application to the Government Grant Committee of the Royal Society of London for aid towards the expenses of the Observatory has likewise been unsuccessful.

The refusal of assistance by the London Committees may be partly due to the fact that there are many claims on the funds which they administer, but it appears also to be very largely due to a want of proper knowledge of what has been done, and what may be reasonably expected to be done by the Observatory, there being no observatory in these islands that can compete with the Ben Nevis Observatory for the accuracy and intrinsic value of the hourly observations; and absolutely no pair of stations anywhere in the world that can be named alongside the Observatory and the station at Fort William, as contributing data in furtherance of our knowledge of storms and the science of weather generally.

The Directors have given much time and thought to the affairs of the Observatory during the past four years, and to many of them it has been a cause of considerable personal expenditure, for the expenses connected with frequent visits made to Ben Nevis for the selection of the site, during the building and equipment of the Observatory, and its subsequent inspections, have in no case been charged against the Observatory funds, but have been borne by the individual Directors. Not only so, but it has happened more than once that some one of the Directors has placed considerable sums to the credit of the Observatory in the bank, to enable the work to go on without an hiatus.

The time has now arrived when it is necessary to place the Observatory on some permanent footing, but before another appeal is made to the public, the Council of this Society has resolved to urge the claims of the Observatory on the consideration of the Government, and to ask for a substantial donation towards the annual expenses. In this, I feel sure, the Council will have the support, not only of the Fellows of the learned Societies of Scotland, but of the general public.

In the infancy of science it was possible, with simple and inexpensive appliances, to discover important facts which were lying as it were on the surface, but no discoveries can now be expected except by rising high above the surface, as in the case of Ben Nevis, or descending far below it, as in the case of the exploration of our deep lochs by means of a steam vessel and the most recent apparatus for sounding, dredging, and taking temperatures; the consequence is that such scientific investigations cannot be undertaken even by scientific men with private fortunes, and hence follows the necessity for Government assistance.

From the estimates it appears—

1. That the Royal Society of London and five or six other learned societies are accommodated in Burlington House free of rent.

2. That the Royal Geographical Society receives a grant of £500 annually.

3. That there is an annual grant of £15,000 for meteorological purposes, administered by a committee of the Royal Society of London, £1000 of which is to be devoted to original investigations.

4. That a sum of £4000 is administered by the Government Grant Committee of the Royal Society of London for scientific research, and although that society is, in a sense, the representative society of the United Kingdom, still its Council and Committees are of necessity composed of Fellows resident in, or within accessible distance of, London, the Fellows resident in Scotland being practically excluded from active participation in the management of the Society.

5. That the Marine Biological Association has recently received £5000 from the Government, and the promise of an annual grant of £500 for five years, towards the establishment of a laboratory of research in England.

6. That in Ireland the Royal Irish Academy receives a grant in aid of £2000 annually, in addition to free accommodation and about £400 annually for allowances and maintenances; the Royal Zoological Society of Ireland receives a grant in aid of £500 annually; and the Royal Dublin Society appears, during the past ten years, to have received considerable sums of money from public funds; in addition there is a large sum granted annually for a national library in Dublin.

7. That, with respect to Scotland, the only grant for scientific purposes in aid of learned societies is £300 annually to the Royal Society of Edinburgh, which is repaid to a department of the Government in the form of rent.

One might well ask what Scotland had done that her learned

Societies and scientific men should be treated so niggardly as compared with those in England and Ireland. It cannot be because she does no scientific work. It is sometimes said, indeed, that in literary matters Scotland, and especially Edinburgh, is a mere shadow of her former self; but in science this is not the case, and it is towards scientific matters that the great ploughshare of human thought and activity is, in this age, directed. I question if any country in the world, taking into consideration its size, can show a better record of scientific work, or a more excellent volume of scientific literature, than Scotland, during the past ten or twenty years.

There can be no doubt that Scotland has a great grievance in the fact that her learned Societies do not receive the same consideration from Government as do similar societies in other parts of the United Kingdom, and in the fact that the administration of all the grants, in which she may be supposed to have a right to participate, is centralised in London and controlled by London Committees. Scotland was refused a representative on the Meteorological Council, where the expenses of the members are paid. Two representatives of this Society are allowed on the committee of sixty members, which distributes the grant of £4000 for research; but when two members of the Council are sent twice a year to London it costs this Society £50 annually,—a considerable charge against its funds. But there is a unanimous opinion among Scottish scientific men that there should be a grant in aid of the learned societies of Scotland, for the purposes of scientific research, analogous to the Government grant of £4000 to the Royal Society of London, which is distributed by a committee in London. Several of the best qualified men in Scotland hesitate to apply for aid from the London committee, and, especially as many of the younger men have been frequently disappointed, there has sprung up a firm belief that the only satisfactory arrangement for the scientific men resident in the northern part of Great Britain is that there should be a grant for scientific research to be distributed by a committee in Scotland, which would be fully conversant with the nature of the investigations to be undertaken, and personally acquainted with the applicants. There can be little doubt that such a grant distributed locally would have the effect of removing many of the barriers which at present impede the progress of science in Scotland.

It is the primary object of our Society to promote the interests of science and literature in Scotland, more especially in all that relates to the extension of the boundaries of knowledge by the discovery of new truths, as distinguished from the making known of old truths. It claims, therefore, the right to memorialise the Government on scientific affairs which it considers of national importance, and in purely Scottish matters it holds that its President and Council, and not the President and Council of the Royal Society of London, should be the advisers of the Government.

The Council has, at various times in the past, called the attention of the Government to the disadvantages under which Scotland laboured in respect of aid to research, but the result has never been satisfactory. In the attempt which the Council is now about to make I would bespeak the co-operation and support of all Scotsmen who believe it to be for the honour and well-being of the country that our scientific institutions should not languish, or our scientific men be discouraged, but that both should be urged to new advances and greater conquests.

HUME MANUSCRIPTS.

Before the reading of the papers the Chairman made the following statement:—

Some reference should, I think, be made to the concluding paragraph of a review of the life of Hume, which recently appeared in the *Scotsman*. It is as follows:—

"Dr Knight has spared no pains to add by his own independent research to the information given in J. Hill Burton's work. He mentions, however, that 'he has not been able to obtain access to the volume of Hume MSS. in the custody of the Royal Society, the Secretary being of opinion that Mr Hill Burton had sufficiently examined these.' Has the Secretary of the Royal Society any such right to bar the way to further genuine research? How does he know that nothing has been omitted and nothing has to be verified? This volume was not given as a present to the Royal Society to keep as a curiosity in a glass case for its peculiar benefit, but given to them as custodiers for the public. The decision of Professor Tait amounts to asserting that this volume is never more to be seen by mortal eye. It is well known that Hume entrusted to the Society his correspondence with Rousseau (after offering it to the British Museum), not merely in order that it might be preserved, but that the world might be able to learn from it the true story of a famous feud. If MSS. of purely literary or philosophical interest are to be locked up jealously in the archives of a Society which is not literary at all, but scientific, the purpose of the depositors is defeated, and the insistance on the private right of the Society becomes a public wrong."

As this article, if unanswered, might give rise to serious misapprehension as to the action of the Society and of our General Secretary, it is advisable to state—

1. The Royal Society of Edinburgh is, by its constitution, quite as much a literary as a scientific society.

2. The General Secretary is not responsible for the decisions of the Council of the Society, except in so far as he is, *ex officio*, one of the twenty-seven members of Council.

3. The Hume MSS. were bequeathed to the Society by Baron Hume in 1838, and they apparently contain papers which Hume in his last will left particular instructions should be destroyed.

4. The Council of the Society has on several occasions since 1838 appointed committees to report as to the access that should be given to these MSS.

5. The Council, recognising the duty of making public the contents of the Hume MSS., and at the same time being anxious to prevent the dissemination of mere hearsay scandal, affecting the characters of men whose descendants are still among us, requested the late Dr Hill Burton—whose competence no one will question—to examine the MSS. and publish them so far as should seem at once necessary and prudent.

6. More recently a committee of experts appointed by the Council (on which are some of the most distinguished literary men in Edinburgh) has for a considerable time been engaged in the laborious work of reperusal of these MSS., with the view of deciding how far further publication of their contents may now be possible.

7. Pending the final decision of this committee, the Rousseau portions of the MSS. have been for some time open to the inspection of, and have been consulted by, investigators.

The following Communications were read:—

2. Astronomical Tables for facilitating the computation of Differential Refraction, for Latitudes 56° and 57°·30. By the Hon. Lord M'Laren.

3. On the Foundations of the Kinetic Theory of Gases.

By Professor Tait.

In a former paper, printed in *Trans. Roy. Soc. Edin.*, 1886, I showed that the recovery of the "special" state by a gas supposed to consist of equal hard spheres takes place, at ordinary pressures and temperatures, in a period of the order of 10^{-9} seconds, at highest.

This forms the indispensable preliminary to the present investigation. For it warrants us in assuming that, except in extreme cases in which the causes tending to disturb the "special" state are at least nearly as rapid and persistent in their action as is the tendency to recovery, a local "special" state is maintained in every region of the space occupied by a gas or gaseous mixture. This may be, and in the cases now to be treated is, accompanied by a common translatory motion of the particles (or, of each separate class of particles) in the region—a motion which at each instant may vary continuously from region to region, and may in any region vary continuously with time.

A troublesome part of the investigation is the dealing with a number of complicated integrals which occur in it, and which (so far as I know) can be treated only by quadratures. All are of the form

$$\int_0^\infty \frac{\nu v^r}{e};$$

where ν is that fraction of the whole number of particles of one kind per cubic unit whose speeds (relatively to those of the same kind, in the same region, *as a whole*) lie between v and $v+dv$; and $1/e$ is the mean free path of a particle whose speed is v. Throughout the paper regard has been had to the fact that e must be treated as a function of v. So long as the particles are of the same kind, or at least of equal mass if of different diameters, such integrals are easy to evaluate; but it is very different when the masses differ in two mixed gases. In what follows, the merely numerical factor of the expression above will be denoted by C_r, so that the value of the expression is, when the masses and diameters are equal, $C_r/n\pi s^2 h^{r/2}$, and the introduction of different diameters merely introduces another factor. Here $3/2h$ is the mean square speed, n the number of particles per cubic unit, and s their common diameter.

When the masses are unequal there will, in general, be different mean free paths for particles of two different kinds, and the integrals cannot be simplified in the above way. In this case the integrals will be expressed as ${}_1\mathfrak{C}_r$, ${}_2\mathfrak{C}_r$.

(1) In my former paper I showed that the Virial equation is, for *equal* hard spheres exerting no molecular action other than the impacts,

$$nP\overline{v^2}/2 = \tfrac{3}{2}p(V - 2n\pi s^3/3),$$

where n is the number of particles, P the mass of one, s its diameter, $\overline{v^2}$ the mean-square speed, p the pressure, and V the volume. The quantity subtracted from the volume is four times the sum of the volumes of the spheres; and I pointed out that this expression exactly agrees in form with Amagat's experimental results for hydrogen, which were conducted through wide ranges of pressure and between 18° C. and 100° C.

In a mixture of equal numbers of two kinds of particles, of diameters s_1, s_2, I find that for s^3 in the above formula we must put

$$\tfrac{1}{4}(s_1{}^3 + 2s^3 + s_2{}^3),$$

where $s = (s_1 + s_2)/2$. Thus the "ultimate volume" is increased if the sizes of the particles differ, though the mean diameter is unaltered.

(2) For the coefficient of viscosity in a single gas the value found is

$$\frac{PnC_1}{3\pi ns^2\sqrt{h}} = \frac{\rho\lambda}{\sqrt{h}}0{\cdot}412,$$

where ρ is the density, and λ the mean free path. The product $\rho\lambda$ is the same at all temperatures, so that the viscosity is as the square root of the absolute temperature.

(3) The steady linear motion of heat in a gas is next considered, temperature being supposed to be higher as we ascend, so as to prevent complication by convection. It is assumed, as the basis of the inquiry, that :—

Each horizontal layer of the gas is in the "special" state, compounded with a vertical translation which is the same for all particles in the layer.

The following are the chief results :—

(*a*) Since the *pressure* is constant throughout, we have

$$p = \frac{\mathrm{P}n}{2h},$$

so that n/h is constant.

(*b*) Since the motion is steady, no *matter* passes (on the whole) across any horizontal plane. This gives for the speed of translation of the layer at x,

$$\alpha = \int_0^\infty v\left(\frac{dn}{dx}\Big/n + \frac{dv}{dx}\Big/v\right)v/3e.$$

(*c*) Equal amounts of *energy* are (on the whole) transferred across unit area of each horizontal plane, per unit of time. The value is

$$\mathrm{E} = -\frac{\mathrm{P}}{6}\int^\infty nvv^3\left(\left(\frac{dn}{dx}\Big/n + \frac{dv}{dx}\Big/v\right)\Big/e - 5\alpha/v\right).$$

By the above value of p, and its consequence as to the ratio n/h, these expressions become

$$\alpha = \frac{dh}{dx}h^{-\frac{5}{2}}\frac{\mathrm{P}}{6p\pi s^2}\left(\frac{5}{2}\mathrm{C}_1 - \mathrm{C}_3\right) = \frac{dh}{dx}h^{-\frac{5}{2}}\frac{\rho\lambda}{p}\,0{\cdot}06,$$

$$\mathrm{E} = \frac{dh}{dx}h^{-\frac{5}{2}}\frac{\mathrm{P}}{6\pi s^2}\left(\frac{25}{4}\mathrm{C}_1 - 5\mathrm{C}_3 + \mathrm{C}_5\right) = \frac{dh}{dx}h^{-\frac{5}{2}}\rho\lambda\,0{\cdot}45.$$

Since E is constant, by the conditions, we see that α also must be constant. Hence as $h\tau$ (where τ is absolute temperature) is constant, we have $\tau^{\frac{1}{2}}\frac{d\tau}{dx}$ constant, or

$$\tau^{\frac{3}{2}} = \mathrm{A} + \mathrm{B}x,$$

which, when the terminal conditions are assigned, gives the steady distribution of temperature. The motion of the gas is analogous to that of liquid mud when a scavenger tries to sweep it into a heap. The broom produces a general translation which is counteracted by the gravitation due to the slope, just as the translation of the gas is balanced by the greater number of particles escaping from the colder and denser layers than from the warmer and less dense.

In thermal foot-minute-centigrade measure, the conductivity of air, at one atmosphere and ordinary temperatures, appears from the above expressions to be about

$$\sqrt{\frac{\tau}{274}}3\,.\,10^{-5},$$

or about 1/28,000 of that of iron. No account, of course, is taken of rotation or vibration of individual particles.

4. Fog Bow observed on Ben Nevis, 22nd October 1886. By R. T. Omond, Supt. B.N.O.

(See *Proceedings* for June 20, below.)

5. Temperatures at Different Heights above Ground at Ben Nevis Observatory. By R. T. Omond, Supt. B.N.O.

During part of the recent summer (1886), in addition to the ordinary temperature observations here, a set of readings were taken in a Stevenson screen, at a height of 112 inches above ground—that is, at about two and a half times the standard elevation of 48 inches. The high level screen was mounted at the top of a stand used to carry the thermometers in winter, and consisting of two stout upright posts or battens, with cross bars between them at every 2 feet or so; the screen is placed with the lower edge of the back resting on one bar, and is tied to the one above it or to the side posts. The screen used on this stand is smaller than the standard Stevenson screen, but it is constructed in exactly the same manner, with double louvred sides and the bottom open. It measures inside 10 inches broad by 6 inches deep and 15 inches high. The low level screen, which is mounted on four legs in the usual manner, is about the ordinary size; it measures 15 by 10 by 15 inches. Readings of both sets of thermometers (high and low) were taken hourly during part of July and the whole of August. During the month of August, as well as these shade temperatures, readings were taken of a black bulb *in vacuo*, belonging to Mr H. N. Dickson, and kindly lent by him for the purpose. In this instrument the thermometer inside the glass globe, instead of being a maximum, is a common thermometer, with the bulb blackened in the usual way. The hourly readings of this instrument indicate the solar radiation at the time of observation, instead of, as in the usual maximum black bulb, giving only the greatest intensity since being last set. In the first column of the table, at the end, the mean hourly values of this black bulb for the month of August are given. It is noteworthy that the maximum is almost exactly at twelve noon; but, as Greenwich time is used, it must be borne in mind that the mean time of the sun's meridian passage is about 12 hours 20 minutes, the longitude being some 5° west. In the second and third columns respectively of the table are the high and low level shade temperatures, and in the fourth the difference between them, for the month of August also. The values of the fourth column are shown graphically in the highest line of the diagram. The average temperature for the whole day is the same in both cases, but the upper thermometer has a slightly less daily range, reading about one-tenth of a degree F. higher at night, and from one to two tenths lower in the afternoon. The result is exactly what would be expected, the upper thermometer being further removed from the ground and less exposed to its radiation, while the smallness of the differences between the two can be accounted for by the exceedingly bad weather of last August: during the whole month there were only two fine days. A comparison of the readings on these two days (the 19th and 22nd), shows a much greater difference. In the fifth column of the table, the mean differences of the high and low level thermometers on the two days is given, and these numbers are also shown in the lower part of the diagram by the line which has the greatest range. Two days is of course far too short a period to give a satisfactory average and a smooth curve, but the general aspect of the line shows that, from about sunrise till noon, the upper thermometer reads distinctly above the lower, from noon to sunset distinctly below it, and during the night slightly above it again. An examination of the black bulb temperatures for these two days shows nothing calling for special note. The range is greater than in the monthly average, but the curve is substantially the same, except for a slight lagging of the maximum point, but the period is too short to say anything definite about this. As a contrast to these fine days, I have computed the mean differences on nine days of continuous fog or mist. These differences are given in column six of the table, and are shown by the line in the lower part of the diagram with the least range. Here, though there is still a tendency of the upper thermometer to read higher at night and lower in the afternoon, the differences are very small;

Hours.	Black Bulb *in vacuo.*	Temp. 48 inches above ground.	Temp. 112 inches above ground.	Diff. of high from low temp.	Mean diff. of 2 fine days.	Mean diff. of 9 foggy days.
Mdnt.	38·8	39·2	39·4	+0·2	+0·2	+0·1
1	38·7	39·1	39·2	+0·1	+0·2	+0·1
2	38·4	38·8	38·8	0·0	+0·1	0·0
3	38·4	38·7	38·8	+0·1	0·0	0·0
4	38·1	38·5	38·6	+0·1	+0·7	+0·1
5	38·8	38·4	38·5	+0·1	+0·7	0·0
6	43·6	38·4	38·6	+0·2	+2·1	0·0
7	47·4	38·9	39·0	+0·1	+1·0	0·0
8	53·6	39·2	39·2	0·0	+0·6	0·0
9	57·9	39·6	39·5	−0·1	+0·4	0·0
10	60·7	39·8	39·8	0·0	+0·6	−0·1
11	64·5	40·3	40·2	−0·1	+0·1	−0·2
12	67·0	40·9	40·7	−0·2	0·0	−0·1
13	64·3	41·3	41·1	−0·2	−0·3	−0·1
14	62·3	41·7	41·5	−0·2	−0·7	−0·2
15	62·2	41·8	41·7	−0·1	−0·6	0·0
16	58·1	42·0	41·8	−0·2	−1·0	−0·1
17	52·5	41·7	41·6	−0·1	−0·4	0·0
18	46·8	41·2	41·2	0·0	+0·1	0·0
19	43·1	40·7	40·7	0·0	+0·2	0·0
20	40·7	40·6	40·6	0·0	0·0	+0·1
21	39·9	40·4	40·5	+0·1	+0·4	+0·1
22	39·8	40·3	40·4	+0·1	+0·3	0·0
23	39·5	39·9	40·0	+0·1	+0·2	+0·1
Mdnt.	39·2	39·6	39·7	+0·1	0·0	+0·1
Mean.	49·0	40·1	40·1	0·0		

at only two hours do they exceed one-tenth of a degree. This small difference on the foggy days fully bears out what I have formerly observed on Ben Nevis, that in a saturated atmosphere—with mist or fog present, it makes no practical difference how the thermometers are placed, so long as the air can reach them at all and they are shaded from the direct rays of the sun.

Differences of High from Low Thermometers.

[illegible]

As the surface of the hill top consists entirely of broken rock without soil or vegetation, it seems probable that a large amount of the difference between the two thermometers is directly due to ground radiation. On a calm day with bright sunshine the stones get so heated as to be disagreeable to handle, but there were unfortunately no such days last August; still, the radiation of heat during the day and of cold at night during ordinary clear weather must be considerable, and the lower thermometer was much nearer this source of heat and cold than the upper. It should, however, be borne in mind that, as the screens are only open below and the bulbs of the thermometers raised about four inches above the lower edge of the sides, they can only receive and transmit radiation from and to that part of the ground that would be visible to an eye placed where the bulbs are ; and that in theory the greater distance of the upper thermometer from the ground would be exactly counterbalanced by the larger area capable of acting on it, and the radiation effects would be the same in both cases. The thermometers were so mounted that the difference in the size of the screens made little difference in the exposure to ground radiation. If the screens themselves get heated and cooled by ground radiation, they would correspondingly heat and cool the air as it passed through the louvres, and thus affect the thermometers. In this latter case the heating and cooling ought to be inversely as the square of their distances from the ground—that is, the lower screen should be affected nearly six times as much as the upper.

The afternoon difference may also be due to convection currents arising from the heated ground; these would affect the lower thermometer more than the upper, but it is difficult to see how any such cause could give the morning difference, in which the upper thermometer reads above the lower. It is possible, however, that in fine weather the layer of air next the ground is so much cooled by contact with the ground that there is a continuous gradient of temperature rising with height above ground, at least as high as the level of the upper screen. I hope to repeat the experiment under more favourable conditions of weather, and also, if possible, when the ground is covered with snow.

PRIVATE BUSINESS.

Messrs Asutosh Mukhopadhyay, M.A., &c., Joseph James Coleman, and John James Burnet were balloted for, and declared duly elected Fellows of the Society.

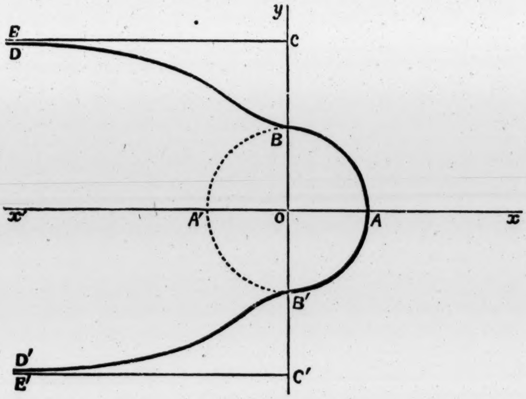
Monday, 20th December 1886.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Motion of Compound Bodies through Liquid. By the Rev. H. J. Sharpe, M.A. Communicated by the President.

It is well known that the solution of the mathematical problem of the motion of liquid arising from the motion through it of solids has been effected in comparatively few instances, and in these, such as the sphere and the ellipsoid, the surface is defined by a single equation. In the following investigation, however, we seem naturally to come across cases where the solid might be described as "a compound body," consisting as it does of parts defined by different equations. It is possible that the method employed might lead to the solution of analogous problems in Sound, Heat, or Electricity.



DBAB′D′ is the trace on the plane of the paper of a material cylindrical surface, which is supposed to move through an infinite mass of water in direction OA with a velocity which at the instant considered is c. BAB′ is a semicircle with centre O, and the other parts of the curve are symmetrical with respect to OA. If $OA = a$, the polar equation of BD, referred to OA as prime radius, is supposed to be

$$\theta - \frac{\pi}{2}\frac{r}{a}\sin\theta + \frac{2}{1.2.3}\frac{a^2}{r^2}\sin 2\theta - \frac{2}{3.4.5}\frac{a^4}{r^4}\sin 4\theta + \frac{2}{5.6.7}\frac{a^6}{r^6}\sin 6\theta - \&c. = 0 \quad . \quad (1)$$

Then the resulting irrotational motion of the liquid, supposed to be in two dimensions only, can be completely determined. It may be remarked that the series in (1), as well as all the like series in this paper, can be presented in a finite form, but, as the results are rather complicated, perhaps it will be better to leave them as they are. It does not matter whether we suppose the solid moving with velocity c through the liquid at rest at infinity, or whether we suppose the solid at rest, and the liquid moving past it with a velocity which at infinity is $-c$. For simplicity, we shall suppose the latter case. First, considering the reflection of the liquid motion only from the semicircle BAB′, let u and v be the velocities of the fluid at any point $P(x, y)$ or (r, θ) of the fluid in the plane of the paper. Let us assume

$$u = \Sigma\frac{a_m}{r^m}\cos m\theta - c\,, \qquad v = \Sigma\frac{a_m}{r^m}\sin m\theta\,,$$

where a_m, &c., are arbitrary constants. It is well known that these expressions satisfy the hydrodynamical equations.

Then the velocity in the directions OP is $u\cos\theta + v\sin\theta$, and

$$u\cos\theta + v\sin\theta = \Sigma\frac{a_m}{r^m}\cos(m-1)\theta - c\cos\theta\,.$$

Now expand $\cos\theta$ by Fourier's theorem in a series of cosines of even multiples of θ between the limits 0 and $\frac{1}{2}\pi$ of θ, which expansion we observe will be true even *at* both limits. Then

$$u\cos\theta + v\sin\theta = \Sigma\frac{a_m}{r^m}\cos(m-1)\theta - \frac{2c}{\pi} + \frac{4c}{\pi}\sum_{n=1}^{n=\infty}\cos 2n\theta\,.\,\frac{\cos n\pi}{4n^2-1}\,.$$

Now determine a_m, &c., by the condition that $u\cos\theta + v\sin\theta$

shall vanish when $r=a$; we thus get $m=2n+1$ and the following series of equations :—

$$\frac{a_1}{a}-\frac{2c}{\pi}=0\,, \qquad \frac{a_5}{a^5}+\frac{4c}{\pi}\frac{1}{3.5}=0\,,$$

$$\frac{a_3}{a^3}-\frac{4c}{\pi}\frac{1}{1.3}=0\,, \qquad \frac{a_7}{a^7}-\frac{4c}{\pi}\frac{1}{5.7}=0\,, \text{ \&c.}$$

We thus get $u+c=$

$$\frac{2c}{\pi}\left\{\frac{a}{r}\cos\theta+\frac{2}{1.3}\frac{a^3}{r^3}\cos 3\theta-\frac{2}{3.5}\frac{a^5}{r^5}\cos 5\theta+\frac{2}{5.7}\frac{a^7}{r^7}\cos 7\theta-\text{\&c.}\right\}$$

$$v=\frac{2c}{\pi}\left\{\frac{a}{r}\sin\theta+\frac{2}{1.3}\frac{a^3}{r^3}\sin 3\theta-\frac{2}{3.5}\frac{a^5}{r^5}\sin 5\theta\right.$$
$$\left.+\frac{2}{5.7}\frac{a^7}{r^7}\sin 7\theta-\text{\&c.}\right\}.$$

It is well known that if we transform from polar to rectangular co-ordinates, the above equations can be expressed in the form

$$u=f'(x+iy)+f(x-iy),$$
$$v=i\{f'(x+iy)-f'(x-iy)\},$$

and when presented in this form we know that the equation of the stream lines is

$$f(x+iy)-f(x-iy)=\text{constant}.$$

Determining in the particular case considered the form of the function f, and then transferring back again to polar co-ordinates, we find for the equation of the stream lines

$$\theta-\frac{\pi}{2}\frac{r}{a}\sin\theta+\frac{2}{1.2.3}\frac{a^2}{r^2}\sin 2\theta$$
$$-\frac{2}{3.4.5}\frac{a^4}{r^4}\sin 4\theta+\frac{2}{5.6.7}\frac{a^6}{r^6}\sin 6\theta-\text{\&c.}=\text{constant}.$$

For the particular stream line which passes through the point B, the above must be satisfied by $r=a$, $\theta=\frac{1}{2}\pi$, and in this case the constant $=0$, and we get equation (1), which is strictly a stream line, but which may evidently be considered as a material curve joined to AB at B. It is evident that CE is an asymptote to this curve, where $OC = 2a$, touching the curve at the point $r = \infty$, $\theta = \pi$. We are concerned with values of θ only from 0 to π. We can readily show that the curve BD touches the circle at B. For in (1) putting $a + \delta r$ for r, and $\frac{1}{2}\pi + \delta\theta$ for θ, we get

$$\delta\theta - \frac{\pi}{2a}\delta r - 2\delta\theta\left(\frac{1}{1.3} + \frac{1}{3.5} + \frac{1}{5.7} + \&c.\right) = 0\,,$$

or $\delta r = 0$. If we retained terms involving $\delta\theta^2$, we could readily prove that a is the radius of curvature at B, and that C is the centre of curvature, so that there must be a point of contrary flexure between B and D. As before remarked, DBAx is a stream line. Thus, as we pass along DB, (1) is its equation. When we get on to the circle BA, we *should* get, putting $r = a$,

$$\theta - \tfrac{1}{2}\pi \sin\theta + \frac{2}{1.2.3}\sin 2\theta - \frac{2}{3.4.5}\sin 4\theta + \frac{2}{5.6.7}\sin 6\theta - \&c. = 0\,,$$

and this *should* be true from $\theta = 0$ to $\theta = \frac{1}{2}\pi$. But if we were to expand $\theta - \frac{1}{2}\pi \sin\theta$ by Fourier's theorem in a series of sines of even multiples of θ between the limits 0 and $\frac{1}{2}\pi$ of θ, we should get exactly the above series, which verifies the work, and we may observe that the expansion would be true even *at* the limits, for $\theta - \frac{1}{2}\pi \sin\theta$ vanishes when $\theta = 0$ and $\theta = \frac{1}{2}\pi$.

It will be found on examination that if AB, instead of being a quadrant, were any part of the semi-circumference, a similar proposition would hold good.

It can also be shown that if AB, instead of being a quadrant of a circle, were a quadrant of an *ellipse*, a like proposition could be established. I will give a slight sketch of the proof. Suppose AB to be part of an ellipse, and suppose it to be moving parallel to OA with a velocity V. Let u, v be the resulting absolute velocities of the fluid at any point (x, y). Then we may take

$$u = \frac{d\phi}{dx}\,, \qquad v = \frac{d\phi}{dy}\,, \qquad \frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0 \quad . \quad . \quad . \quad . \quad (2)$$

Now put $x = c\cosh\beta\cos\alpha$, $y = c\sinh\beta\sin\alpha$,
where c is a constant, sinh and cosh are the hyperbolic sine and cosine, and α, β are new so-called curvilinear co-ordinates. Then (2) becomes

$$\frac{d^2\phi}{d\alpha^2} + \frac{d^2\phi}{d\beta^2} = 0\,.$$

Assume for a solution of this equation

$$\frac{d\phi}{d\alpha} = \Sigma a_m \epsilon^{-m\beta} \sin m\alpha\,, \qquad \frac{d\phi}{d\beta} = \Sigma a_m \epsilon^{-m\beta} \cos m\alpha + \mathrm{C}\,,$$

where C is a constant, and a_m, &c. are constants to be determined. We may observe that the velocity at infinity normal to the ellipse $\beta = \text{const.}$ is $d\phi/\mathrm{P}d\beta$ where $\mathrm{P}^2 = \frac{1}{2}c^2(\cosh 2\beta - \cos 2\alpha)$, so that whether C is zero or finite, with the above assumption, the whole velocity at infinity is zero. Now suppose $\beta = \beta_1$ gives the solid cylinder, then it will be found that the condition for reflection of the fluid motion at $\beta = \beta_1$ is

$$\Sigma a_m \epsilon^{-m\beta_1} \cos m\alpha + \mathrm{C} = \mathrm{V}c \sinh \beta_1 \cos \alpha\,. \quad . \quad . \quad . \quad . \quad (3)$$

If the ellipse is complete, (3) must hold for all values of α from 0 to π, and then we shall find that we must have $\mathrm{C} = 0$ and $m = 0$. This leads to the solution given in Art. 88 (*d*) of Lamb's *Motion of Fluids.* But if the reflection takes place at only a quadrant of the ellipse, (3) admits of another solution. Expand in (3) $\cos\alpha$ by Fourier's Theorem in a series of cosines of even multiples of α from $\alpha = 0$ to $\alpha = \frac{1}{2}\pi$, and we get

$$\Sigma a_m \epsilon^{-m\beta_1} \cos m\alpha + \mathrm{C} = \frac{2}{\pi} \mathrm{V}c \sinh \beta_1 \left\{ 1 - \Sigma_1^{\infty} \cos 2n\alpha \frac{2 \cos \pi n}{4n^2 - 1} \right\}.$$

Now let $\qquad \mathrm{C} = \dfrac{2}{\pi} \mathrm{V}c \sinh \beta_1, \qquad m = 2n,$

and

$$a_{2n} \epsilon^{-2n\beta_1} = -\frac{4}{\pi} \mathrm{V}c \sinh \beta_1 \frac{\cos n\pi}{4n^2 - 1}\,,$$

and we have a solution exactly analogous to the case of the circle. It will be found also that instead of taking a quadrant of an ellipse, we might take AB to be any arc and still get a similar proposition.

We will next take a case of motion in three dimensions. We will suppose a solid hemisphere BAB′ of radius a moving through an infinite mass of liquid in direction OA, with a velocity which at the instant considered is V, the velocity of the liquid at infinity being zero. The motion of the liquid is supposed to be in planes through OA, and to be symmetrical round it. We will suppose a velocity $-V$ in direction Ox to be impressed upon the solid and fluid, so as to bring the former to rest, and let ϕ be the velocity potential of the fluid motion *relative* to the solid at any point P whose polar coordinates are r, θ. Then ϕ satisfies Laplace's equation $\Delta^2\phi = 0$, which changed to polar coordinates becomes

$$\frac{d^2\phi}{dr^2} + \frac{2}{r}\frac{d\phi}{dr} + \frac{1}{r^2 \sin\theta}\frac{d}{d\theta}\left(\sin\theta\frac{d\phi}{d\theta}\right) = 0\,.$$

We may take for a solution of this equation

$$\phi = -Vr\cos\theta + a_0\frac{a}{r} + a_2 P_2\frac{a_3}{r^3} + a_4 P_4\frac{a_5}{r^5} + \&c.,$$

where P_2, P_4, &c., are Legendre's coefficients of even order, and a_0, a_2, &c., are constants to be determined. The first term is introduced because the velocity at infinity parallel to OA must be $-V$. We can determine a_0, a_2, &c., by the condition that $d\phi/dr$ must vanish, when $r = a$.

This gives us

$$V\cos\theta = -\frac{1}{a}(a_0 + 3a_2P_2 + 5a_4P_4 + \&c.)\ . \qquad (4)$$

This must be true from $\theta = 0$ to $\theta = \frac{1}{2}\pi$.

Now $\cos\theta$ can be expanded in a series of Legendre's coefficients of even orders between the above limits. Putting $\cos\theta = x$,

$$\int_0 P_m P_m dx = \frac{1}{2m+1} \text{ and} \int_0^1 P_m P_n dx = 0\,,$$

which last is true if both m and n are even. We then get

$$a_0 = -\tfrac{1}{2}aV\,, \qquad a_2 = -\tfrac{5}{24}aV\,,$$

and generally

$$a_r = (-1)^{\frac{1}{2}r} aV\frac{2r+1}{r+1} \times \frac{3.5\ldots(r-3)}{2.4\ldots(r+2)}$$

It will be found that the differential equation to the stream lines can be expressed in the form (putting $\frac{a}{r} = \rho$ for shortness),

$$(1-x^2)\left\{-V\frac{a}{\rho^3} - a_2\rho\frac{dP_2}{dx} + a_4\rho^3\frac{dP_4}{dx} + \&c.\right\}d\rho$$

$$-\left\{\frac{1}{\rho^2}Vax + a_0 + 3a_2P_2\rho^2 + 5a_4P_4\rho^4 + \&c.\right\}dx = 0\,. \quad . \quad (5)$$

Remembering the known relation

$$\frac{d}{dx}\left\{1-x^2)\frac{dP_n}{dx}\right\}+n(n+1)\,P_n=0,$$

and examining the general term of (5), we see that (5) is an exact differential. Therefore integrating we shall get for the equation of the stream lines

$$(1-x^2)\left\{\frac{aV}{2\rho_2}+a_2\frac{\rho^2}{2}\frac{dP_2}{dx}+a_4\frac{\rho^4}{4}\frac{dP_4}{dx}+\&c\cdot\right\}-a_0x=C$$

For the particular stream line, which passes through the point B, the above must be satisfied by $\rho=1$, $\theta=\frac{1}{2}\pi$ or $x=0$, and therefore $C=\frac{aV}{2}$, so that its equation is

$$(1-x^2)\left\{\frac{aV}{2\rho^2}+a_2\frac{\rho^2}{2}\frac{dP_2}{dx}+a_4\frac{\rho^4}{4}\frac{dP_4}{dx}+\&c.\right\}-a_0x=\frac{aV}{2}\quad.\quad(6)$$

For distant points ρ is small, therefore for such points neglecting all the positive powers of ρ in the above, we can readily prove that $y=\sqrt{(2)}a$ is an asymptote, and that the curve lies below it. It is interesting to see whether at B the curve goes up or down. To this end, in (6) put $a+\delta r$ for r and $\frac{1}{2}\pi+\delta\theta$ for θ, expand and retain not beyond the first power of δr and the second power of $\delta\theta$. It will be found that we shall get

$$2\delta r-a\delta\theta^2+a\Bigg[-1+\tfrac{5}{8}+\tfrac{9}{64}+\&c.$$
$$+\frac{1}{n}\,\frac{4n+1}{2n+1}\,\frac{3.5\ldots(2n-3)}{2.4\ldots(2n+2)}\,\frac{3.5\ldots(2n+1)}{2.4\ldots(2n-2)}+\&c.\Bigg]\delta\theta=0\quad.\quad(7)$$

In order to get the terms after the 3rd in this series, we must take n from 3 to infinity. There seems to be no doubt that the value of the series in the square brackets is zero, although it is rather difficult to prove it algebraically, for the expansion (4) is true even *at* the limits. Moreover the expansion (4) is never differentiated, therefore $d\phi/dr$ is zero for $r=a$ even *at* the point B, therefore, as in the case of the circular cylinder, the stream line through B must *touch* the circle at B; assuming therefore that the series in (7) is zero, the remaining terms show us that the curvature at B is upwards, and the radius of curvature equal to a. The same figure therefore as was employed for the circular cylinder will do for this case, provided that we make $OC=\sqrt{(2)}a$ instead of $2a$.

2. Note on Knots on Endless Cords. By A. B. Kempe, Esq. Communicated by Prof. Tait. (Plate I.)

1. Each crossing divides the cord into two loops.

2. Any other crossing either lies on both these loops, say is *linked* to the former crossing, or on one loop only, say is *not linked* to the former crossing.

3. It can be shown without difficulty that if crossing *a* is linked to crossing *b*, then crossing *b* is linked to crossing *a*; and therefore, also, if crossing *a* is not linked to crossing *b*, crossing *b* is not linked to crossing *a*.

4. Hence *pairs* of crossings are of two sorts, viz., linked and unlinked.

5. We have the two following fundamental laws :—

(*a*) The number of crossings linked to each crossing is even.

(*b*) If two crossings are not linked to each other, the number of crossings linked to both is even.

6. We may represent a knot diagrammatically thus :—

Represent the crossings by small circles or *nuclei.*

Join pairs of nuclei which represent pairs of linked crossings by lines or *links.*

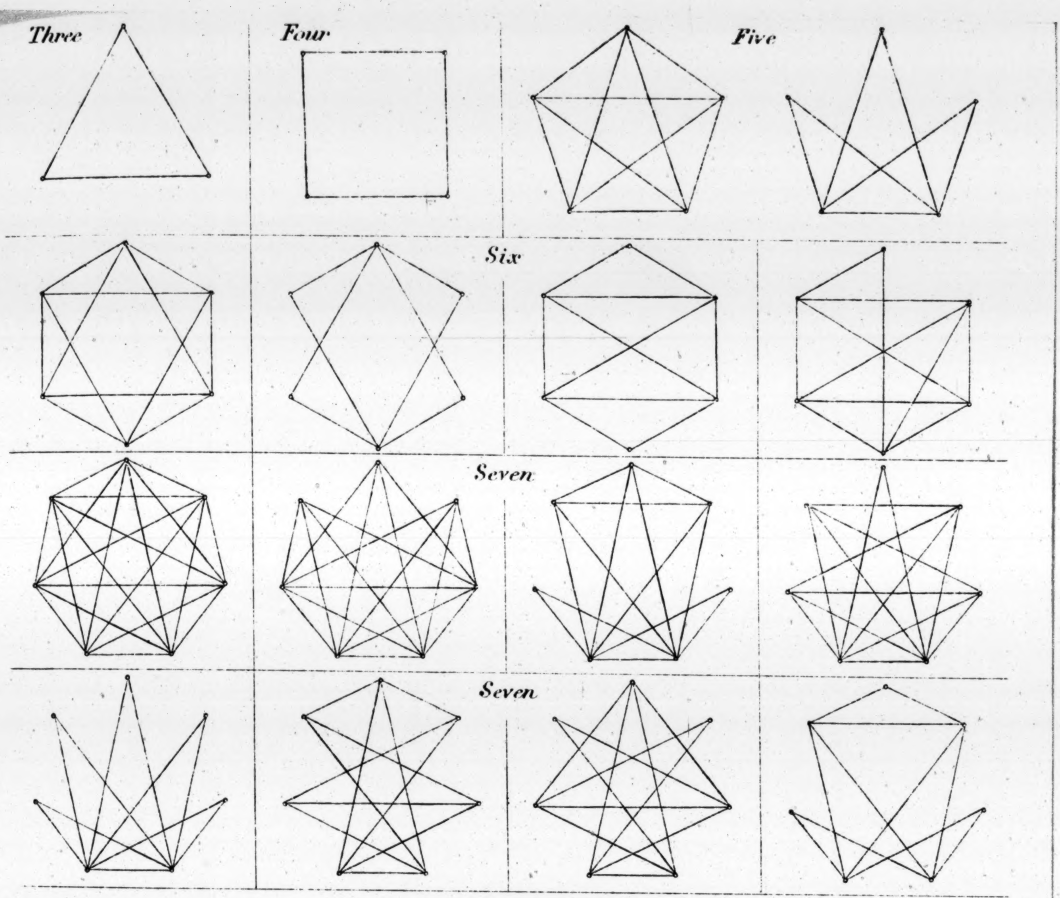
No lines are to be drawn in the case of unlinked pairs of crossings.

7. In these diagrams, in conformity with sec. 5, the number of links proceeding from a nucleus must be even, and if two nuclei are not joined by a link, the number of nuclei joined by links to both must be even.

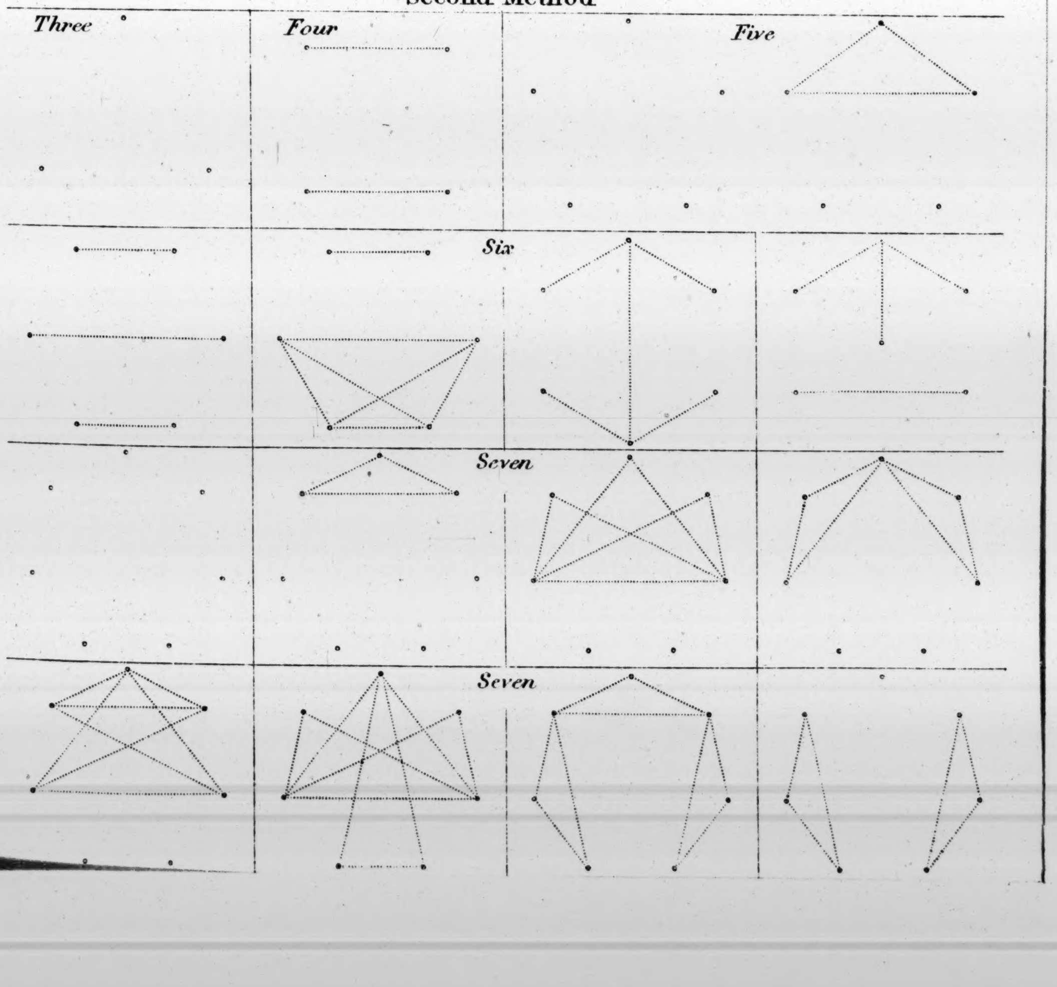
8. This mode of representing knots has the advantage of indicating the degree of complexity of the various knots. Thus, the diagram representing two distinct knots on the same string will consist of two entirely detached portions, and nugatory crossings will be represented by nuclei having no links proceeding from them.

9. In the plate the various knots of *three*, *four*, *five*, *six*, and *seven* crossings are indicated in outline on this plan.

10. It will occasionally be convenient to represent pairs of crossings which are *unlinked* by pairs of nuclei joined by a dotted line or link, pairs of crossings which are linked not being joined at all.



Second Method



The advantage of this mode of representation is that it involves a smaller number of lines than the former mode, and thus the diagrams are simpler and clearer.

11. In the lower half of the plate the same knots are indicated again, but on this new plan. [By an oversight the last, and the last but two, of the seven-folds have been interchanged in this part of the diagram.]

3. On the Ring-Waves produced by throwing a Stone into Water. By Sir W. Thomson.

(Printed in full in the *Philosophical Magazine*, Jan. 1887.)

4. On the Waves produced by a Ship advancing uniformly into Smooth Water. By the Same.

(*Phil. Mag.*, Jan. 1887.)

5. Expansion of Functions in terms of Linear, Cylindric, Spherical, &c., Functions. By P. Alexander, M.A. Communicated by Dr T. Muir.

(This paper is printed in the *Transactions.*)

6. On Even Distribution of Points in Space. By Prof. Tait.

(*Abstract.*)

The question raised is very closely connected with § 6 of my paper on the *Kinetic Theory of Gases* (*Trans. R.S.E.* xxxiii. 71). The result arrived at is that, when (in the thin layer) some particles prevent others from doing their full duty, the formula should be

$$\varepsilon^{-n_1\pi s^2\delta x}, \text{ instead of } 1 - n_1\pi s^2\delta x$$

as given in the text.

Friday, 7th January 1887.

SIR DOUGLAS MACLAGAN, Vice-President, in the Chair.

At the request of the Council, Mr J. J. Coleman gave an Address on Processes of Refrigeration.

The following Papers were laid on the Table:—

1. On the Front and Rear of a Free Procession of Waves in Deep Water. By Sir William Thomson, F.R.S.

PRELIMINARY.

General Problem of Deep-Sea Wave-Motion in two dimensions. *(Infinitesimal Motion.)*

Taking x horizontal, and y vertically downwards; let $(x+\xi, y+\eta)$ be, at time t, the position of the particle whose position at time 0 is (x, y); let Φ denote the velocity-potential at (x, y, t); and let P denote its time-integral, $\int_0^t dt\Phi$. We have

$$\xi = \int_0^t dt\frac{d\Phi}{dx} = \frac{dP}{dx}; \text{ and } \eta = \int_0^t dt\frac{d\Phi}{dy} = \frac{dP}{dy} \quad . \quad . \quad . \quad (1).$$

Let p be the pressure at $(x+\xi, y+\eta)$. (The motion being infinitesimal,) we have

$$p = C + g(y+\eta) - \frac{d\Phi}{dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

or, in virtue of (1),

$$p = C + gy + g\frac{dP}{dy} - \frac{d^2P}{dt^2} \quad . \quad . \quad . \quad . \quad . \quad (3).$$

The kinematical conditions are, the equation of continuity,

$$\frac{d^2P}{dx^2} + \frac{d^2P}{dy^2} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (4);$$

and the boundary equation, in two parts—one relating to the upper surface, the other to the bottom. The latter, for our present case of infinitely deep water, is simply

$$\mathrm{P} = 0 \text{ when } y = \infty \quad . \; . \; . \; . \; . \; . \; . \quad (5).$$

To find the former, or upper-surface kinematical equation, at time t, let it be $y = 0$ at time 0, and let $\mathfrak{h}$ be the height at time t above the level $y = 0$, of the upper-surface particle whose coordinates at time 0 are $(x, 0)$. Remembering that y positive was taken as *downwards*, we have by (1),

$$\mathfrak{h} = -\left(\frac{d\mathrm{P}}{dy}\right)_{y=0} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (6).$$

The most general upper-surface dynamical condition which can be imposed is

$$p_{(y=0)} = f(x, t) \quad . \; . \; . \; . \; . \; . \; . \; . \quad (7),$$

where f denotes an arbitrary function of the two independent variables.

Suppose now the water to be at rest at time 0. It is clear from dynamical considerations that the solution of (4), subject to the conditions (5), (7), (3), is fully determinate: and when it is found, (1) gives the position at time t of the fluid-particle which at time 0 was in any position (x, y); and so completes the solution of the problem.

The particular solution which we are now going to work out to represent a uniform procession of waves commencing at time 0, and produced and maintained by the application of changing pressure to the surface in the neighbourhood of the zero of x, must, as its appropriate form of (7), fulfil the condition

$$p_{(y=0)} = \mathfrak{F}(x) \sin \omega t + \mathrm{F}(x) \cos \omega t \quad . \; . \; . \; . \; . \quad (8),$$

where $\mathfrak{F}(x)$ and $\mathrm{F}(x)$ denote functions which vanish for all large positive or negative values of x.

If we wish to make only a single procession, in the direction of x positive for example, we may take

$$\mathfrak{F}(x) = \mathrm{F}(x - \tfrac{1}{2}g\pi/\omega^2) \quad . \; . \; . \; . \; . \; . \; . \quad (9).$$

A perfectly general formula is easily (by the Fourier-Poisson-Cauchy method) written down to express the value of P; and so, by (1) and (6), the complete solution of the problem: for $\mathfrak{F}$ and F any given arbitrary functions.

It is obvious that, so far as $\mathfrak{h}$ is concerned, the general solution for x any considerable multiple of $\pm l$, and exceeding $\pm l$ by not less than two or three times the wave-length, $2\pi g/\omega^2$, must, for values of t great enough to have let the front of the procession pass the place x, be

$$\left.\begin{aligned} &= \mathfrak{A}\sin\left[\omega t - \frac{\omega^2}{g}(x-f)\right] + \mathrm{A}\cos\left[\omega t - \frac{\omega^2}{g}(-\mathrm{f})\right] \\ &\qquad\qquad\qquad\qquad\text{for } x \text{ positive,} \\ &\text{and} \\ \mathfrak{h} &= \mathfrak{A}\sin\left[\omega t - \frac{\omega^2}{g}(-x+f)\right] - \mathrm{A}\cos\left[\omega t - \frac{\omega^2}{g}(-x+\mathrm{f})\right] \\ &\qquad\qquad\qquad\qquad\text{for } x \text{ negative,} \end{aligned}\right\} (10);$$

where $\mathfrak{A}$ and f denote quantities calculable from the form of $\mathfrak{F}$; and A and f similarly from F. Further, it is obvious that the front of each procession will, for any value of t not less than several times the period and not less than several times the time one of the wave-crests takes to travel through a space equal to l, be independent of the particular forms of $\mathfrak{F}$ and F. From the theory of Stokes, Osborne Reynolds, and Rayleigh, we know that it advances at half the speed of a wave-crest; but their theory, so far as hitherto developed, does not teach us the law according to which the front, as it advances, becomes longer and longer in proportion to $\sqrt{t}$, nor even the fact that it does become longer and longer. All the details of this interesting question are exquisitely given in what follows: having been found with great ease for the particular case,

$$\mathrm{F}(x) = 0, \text{ and } \mathfrak{F}(x) = \left\{\frac{(x^2+b^2)^{\frac{1}{2}}+b}{x^2+b^2}\right\}^{\frac{1}{2}} \quad . \quad . \quad . \quad (11),$$

where b denotes a length of any magnitude, which we shall take to be very small in comparison with $2\pi g/\omega^2$, the wave-length. We shall in fact find that

$$p_{(y=0)} = \mathrm{C} + \left\{\frac{(x^2+b^2)^{\frac{1}{2}}+b}{x^2+b^2}\right\}^{\frac{1}{2}} \sin\omega t \quad . \quad . \quad . \quad (12),$$

in the particular processional case of the general equations (1) . . . (6), which we now go on to work out.

Remembering Cauchy and Poisson's discovery that every surface of particles which are in a horizontal plane when undisturbed fulfils the condition of a free upper-surface (so that if all the water above it were annulled the motion of the water remaining below it would be undisturbed), in the case of free waves of infinitely deep water; we see that when $p_{(y=0)} = \text{const.}$, we have also, in our notation, $p = \text{const.}$, for every constant value of y. Hence, looking to (3) above, we must find, in the case of free waves,

$$\frac{gd\mathrm{P}}{dy} = \frac{d^2\mathrm{P}}{dt^2} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (13);$$

for every value of y, and not only at the upper surface $y = 0$. Thanking Cauchy and Poisson for this as a suggestion, but not assuming it without the proof of it which we immediately find; and borrowing now from Fourier* his celebrated "instantaneous plane-source" † solution of his equation $\frac{dv}{dt} = k\frac{d^2v}{dx^2}$ for thermal conduction, assume, as an imaginary type-solution of (4) and (13) for free waves,

$$\frac{1}{(b+y+\iota x)^{\frac{1}{2}}}\epsilon^{\frac{-gt^2}{4(b+y+\iota x)}} \quad . \; . \; . \; . \; . \; . \quad (14),$$

where ι denotes $\sqrt{-1}$. Whence, as a real solution by adding the values of (14) for ι and $-\iota$, and dividing by $\sqrt{2}$,

$$\left.\begin{aligned} \phi(t) &= \frac{1}{r}\left\{ (r+y+b)^{\frac{1}{2}} \cos\frac{gt^2x}{4r^2} + (r-y-b)^{\frac{1}{2}} \sin\frac{gt^2x}{4r^2} \right\} \epsilon^{\frac{-gt^2(y+b)}{4r^2}} \\ \text{where} \qquad r &= [(y+b)^2 + x^2]^{\frac{1}{2}} \end{aligned}\right\} (15).$$

Curves representing calculated results of this solution for free waves were shown at the meeting of the British Association (Section A) at Birmingham in September, and at the last meeting (December 20) of the Royal Society of Edinburgh. To build up of it a solution for a uniformly maintained procession of waves (a double procession it shall be, of equal and similar waves travelling in the two directions from $x = 0$) take

$$\mathrm{P}(t) = \int_0^t dt\phi(t) \quad . \; . \; . \; . \; . \quad (16);$$

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* *Théorie Analytique de la Chaleur.*

† Sir W. Thomson's Collected Papers, vol. ii. p. 46.

and

$$P = -\int_0^t dt \sin \omega t' \mathbf{P}(t-t') = -\int_0^t dt \sin \omega(t-t')\mathbf{P}(t') \quad . \quad (17).$$

Since $\phi(t)$, as we have seen, satisfies (13), $\mathbf{P}(t)$ must satisfy it also. Hence

$$g\frac{d\mathbf{P}(t)}{dy} = \frac{d^2\mathbf{P}(t)}{dt^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (18),$$

for all values of x, y, and t. Now by differentiation of (17) we find, because $\mathbf{P}(0)=0$, and by (16),

$$\frac{dP}{dt} = -\int_0^t dt' \sin \omega t' \frac{d}{dt}\mathbf{P}(t-t') = -\int_0^t dt' \sin \omega t' \phi(t-t') \quad . \quad (19);$$

and differentiating this, we find, because $\phi(0) = (r+y+b)^{\frac{1}{2}}r^{-1}$

$$\frac{d^2P}{dt^2} = -\frac{(r+y+b)^{\frac{1}{2}}}{r}\sin \omega t - \int_0^t dt' \sin \omega t' \frac{d}{dt}\phi(t-t')$$

$$= -\frac{(r+y+b)^{\frac{1}{2}}}{r}\sin \omega t - \int_0^t dt' \sin \omega(t-t')\frac{d^2\mathbf{P}(t')}{dt'^2} \quad . \quad (20).$$

From this and the second form of (17) we find

$$g\frac{dP}{dy} - \frac{d^2P}{dt^2} = \frac{(r+y+b)^{\frac{1}{2}}}{r}\sin \omega t$$

$$-\int_0^t dt \sin \omega(t-t')\left[g\frac{d\mathbf{P}(t')}{dy} - \frac{d^2\mathbf{P}(t')}{dt'^2}\right] \quad . \quad . \quad . \quad (21);$$

whence, by (18)

$$g\frac{dP}{dy} - \frac{d^2P}{dt^2} = \frac{(r+y+b)^{\frac{1}{2}}}{r}\sin \omega t \quad . \quad . \quad . \quad (22);$$

and therefore finally, by (3) above, we have, for the surface pressure,

$$p_{(y=0)} = C + \left\{\frac{(x^2+b^2)^{\frac{1}{2}}+b}{x^2+b^2}\right\}^{\frac{1}{2}}\sin \omega t \quad . \quad (23),$$

as promised in (12) above.

To work out our solution, remember that dP/dt is the velocity-potential of the motion; and calling this Φ, we find, by (19),

$$\Phi = -\int_0^t dt' \sin \omega(t-t')\phi(t) \quad . \quad . \quad . \quad . \quad (24);$$

and by (22), (3), and (2) we find

$$\eta = \frac{1}{g}\left\{\frac{d\Phi}{dt} + \frac{(r+y+b)^{\frac{1}{2}}}{r}\sin\omega t\right\} . \quad . \quad . \quad . \qquad (25).$$

What we chiefly want to know is the surface-value of η, which we have denoted by $-\mathfrak{h}$; and we shall work this out for the case $b=0$. But it is to be remarked that the assumption of $b=0$ does not diminish the generality of our problem, because the motion at at any depth, c, below the upper surface with $b=0$, is the same as the motion at the surface, with $b=c$.

Put now $b=0$ and $y=0$ in (15): we find

$$\phi(t) = x^{-\frac{1}{2}}\left(\cos\frac{gt^2}{4x} + \sin\frac{gt^2}{4x}\right) = \sqrt{\frac{2}{x}}\sin\left(\frac{gt^2}{4x} + \frac{\pi}{4}\right) \quad . \qquad (26).$$

Using this in (24), and putting $\sigma^2 = gt'^2/4x$, we find

$$\Phi = -2\sqrt{\frac{2}{g}}\int_0^{\sqrt{\frac{g}{4x}}} d\sigma \sin\left(\omega t - 2\omega\sqrt{\frac{x}{g}}\sigma\right)\sin\left(\sigma^2 + \frac{4}{\pi}\right) \quad . \qquad (27),$$

$$= \sqrt{\frac{2}{g}}\int_0^{t\sqrt{\frac{g}{4x}}} d\sigma \left\{ \cos\left[\left(\sigma - \omega\sqrt{\frac{x}{g}}\right)^2 - \omega^2\frac{x}{g} + \omega t + \frac{\pi}{4}\right] \right.$$

$$\left. - \cos\left[\left(\sigma + \omega\sqrt{\frac{x}{g}}\right)^2 - \omega^2\frac{x}{g} - \omega t + \frac{\pi}{4}\right]\right\} \qquad (28).$$

Using now the following notation,

$$say\,\theta = \int_0^\theta d\theta \sin\theta^2 ; \qquad cay\,\theta = \int_0^\theta d\theta\cos\theta^2 \quad . \qquad (29),$$

for two integrals which have been tabulated by Airy * through the range from 0 to $5{\cdot}5\sqrt{\frac{\omega}{2}}$ we reduce (28) to

$$\sqrt{\frac{g}{2}}\Phi = \left[cay\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + cay\left(\omega\sqrt{\frac{x}{g}}\right)\right]\cos\left(\frac{\omega^2}{x}x - \omega t - \frac{\pi}{4}\right)$$

$$+ \left[say\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + say\left(\omega\sqrt{\frac{x}{g}}\right)\right]\sin\left(\frac{\omega^2}{g}x - \omega t - \frac{\pi}{4}\right)$$

$$- \left[cay\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - cay\left(\omega\sqrt{\frac{x}{g}}\right)\right]\cos\left(\frac{\omega^2}{g}x + \omega t - \frac{\omega}{4}\right)$$

$$- \left[say\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - say\left(\omega\sqrt{\frac{x}{g}}\right)\right]\sin\left(\frac{\omega^2}{g}x + \omega t - \frac{\omega}{4}\right) . \qquad (30).$$

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* "Tracts" (Undulatory Theory of Optics, last page).

The interpretation of this is eased by putting it into the form

$$\Phi = \sqrt{\frac{2}{g}}\left\{ \mathrm{Q}\cos\left(\frac{\omega^2}{g}x - \omega t - e\right) - \mathrm{R}\cos\left(\frac{\omega^2}{g}x + \omega t - f\right)\right\}. \quad (31).$$

where

$$\mathrm{Q} = \left\{\left[cay\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + cay\left(\omega\sqrt{\frac{x}{g}}\right)\right]^2 + \left[say\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + say\left(\omega\sqrt{\frac{x}{g}}\right)\right]^2\right\}^{\frac{1}{2}}. \quad (32);$$

$$e = \tan^{-1}\frac{say\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + say\left(\omega\sqrt{\frac{x}{g}}\right)}{cay\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + cay\left(\omega\sqrt{\frac{x}{g}}\right)} - \frac{\mu}{4} \quad . \; . \; . \; . \quad (33);$$

$$\mathrm{R} = \sqrt{\frac{2}{g}}\left\{\left[cay\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - cay\left(\omega\sqrt{\frac{x}{g}}\right)\right]^2 + \left[say\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - say\left(\omega\sqrt{\frac{x}{g}}\right)\right]^2\right\}^{\frac{1}{2}} \quad . \quad (34);$$

and

$$f = \tan^{-1}\frac{say\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - say\left(\omega\sqrt{\frac{x}{g}}\right)}{cay\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - cay\left(\omega\sqrt{\frac{x}{g}}\right)} - \frac{\pi}{4} \quad . \; . \quad (35).$$

Now, remembering that $cay\,(\infty) = say\,(\infty) = \sqrt{\frac{\pi}{8}}$, we see that if $\omega\sqrt{\frac{x}{g}}$ is large, and $t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}$ is large positive, we have

$$\mathrm{R} \doteqdot 0, \quad \mathrm{Q} \doteqdot \sqrt{\pi}, \quad e \doteqdot 0 \quad . \; . \; . \; . \; . \; . \quad (36);$$

and therefore

$$\phi \doteqdot \sqrt{\frac{2\pi}{g}}\cos\left(\frac{\omega^2 x}{g} - \omega t\right) \quad . \; . \; . \; . \quad (37)$$

whence, and by (25) with $b = 0$,

$$\mathfrak{h} \doteqdot -\frac{1}{g}\omega\sqrt{\frac{2\pi}{g}}\left[\sin\left(\frac{\omega^2 x}{g} - \omega t\right) - \frac{1}{\omega}\sqrt{\frac{g}{2\pi x}}\sin\omega t\right] \quad . \quad (38);$$

or, since $\omega\sqrt{(x/g)}$ is very large,

$$\mathfrak{h} \doteqdot -\frac{\omega}{g}\sqrt{\frac{2\pi}{g}}\sin\left(\frac{\omega^2 x}{g} - \omega t\right) \quad . \; . \; . \; . \quad (39).$$

This represents a uniform procession of free waves, of which the wave-length, λ, and the wave-velocity, U, are as follows :—

$$\lambda = 2\pi g/\omega^2, \quad U = g/\omega \; . \; . \; . \; . \; . \; . \; . \quad (40).$$

To explain the meaning of "very large" as we have just now used it, let

$x = n\lambda$, which makes $\omega\sqrt{\frac{x}{g}} = \sqrt{2\pi n}$, and $\frac{1}{\omega}\sqrt{\frac{g}{2\pi x}} = 1/4\pi\sqrt{n}$ (41).

Hence the term of (38) omitted in (39) is $1/4\pi\sqrt{n}$ of that retained. And the value of the R, omitted by (36) in (37), is of the order $1/2\sqrt{2n}$ of the Q which is retained, because

$$cay(\infty) - cay(\sqrt{2\pi n}) \doteqdot -\frac{\sin(2\pi n)}{2\sqrt{2\pi n}},$$

and $$say(\infty) - say(\sqrt{2\pi n} \doteqdot \frac{\cos(2\pi n)}{2\sqrt{2\pi n}} \; . \; . \; . \; . \; . \; . \; . \quad (42),$$

when n is very large.

In (36) and its consequence (31), we supposed t so large that $t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}$ is large positive; let us next suppose t so small that it is large negative; that is to say, let

$$t = 2\omega x/g - m\sqrt{\frac{4x}{g}} \; . \; . \; . \; . \; . \; . \; . \quad (43),$$

where m is a large positive numeric. Thus, remarking that $cay(-\theta) = -cay(\theta)$, and $say(-\theta) = -say(\theta)$, we have, by (43) and (41) in (32),

$$Q = \sqrt{\frac{2}{g}}\{[cay(m) - cay(\sqrt{2\pi n})]^2 + [say(m) - say(\sqrt{2\pi n})]^2\}^{\frac{1}{2}} \quad (44);$$

and therefore, when m and n are each very large, $Q \doteqdot 0$. Because n is large we still, as in (36), have $R \doteqdot 0$; and therefore the motion is approximately zero, at any considerable number, n, of wave-lengths from the origin, so long as m in (43) remains large. As time advances, m decreases to 0, and on to $-\infty$: and, watching at the place $x = n\lambda$, we see wave-motion gradually increasing from nothing, till it becomes the regular procession of waves represented by (39); and continues so unchanged for ever after. When $m = 0$, that is to say, at the time

$$t = 2\omega x/g \; . \; . \; . \; . \; . \; . \; . \; . \quad (45),$$

Q has attained half its final value. The point x where this condition is fulfilled at time t may be called the mid-front of the procession. It travels at the velocity $\frac{1}{2}g/\omega$, or half the wave-velocity; which agrees with the result of Stokes.

We may arbitrarily define "the front" as the succession of augmenting waves which pass between the times corresponding to $m = +10$ and $m = -10$ (or any other considerable number instead of 10). Thus the time taken by the front, in passing the place $x = n\lambda$, is $40\omega^{-1}\sqrt{2\pi n}$. The space travelled by the mid-front in this time is $20g\omega^{-1}\sqrt{2\pi n}$, which may, arbitrarily, be defined as the length of the front. It increases in proportion to $\sqrt{n}$; and therefore in proportion to $\sqrt{t}$, as said above. The effect upon phase of the changing waves in the front; due to the fluctuations of e, and to the law of augmentation of Q from zero to its final value; is to be illustrated by calculations and graphic representations, which I hope will be given on a future occasion.

The rear of a wholly free procession of waves may be quite readily studied after the constitution of the front has been fully investigate, by superimposing an annulling surface-pressure upon the originating pressure represented by (12) above, after the originating pressure has been continued so long as to produce a procession of any desired number of waves.

2. Numerical and other Additions to his Paper, read on 6th December 1886, on the Foundations of the Kinetic Theory of Gases. By Professor Tait.

In the case of diffusion, in a long tube of unit section, suppose that we have, at section x of the tube, n_1P_1s and n_2P_2s per cubic unit, with translational speeds α_1 and α_2, respectively. If G_1 be the whole mass of the first gas on the negative side of the section, it is shown that the rate of flow of that gas is

$$\frac{dG_1}{dt} = -P_1\left(n_1\alpha_1 - {}_1\mathfrak{C}_1\frac{dn_1}{dx}\Big/3\right), \text{ \&c.}$$

Obviously

$$\frac{dG_1}{dx} = P_1 n_1, \text{ \&c.}$$

The motion of the layer of P_1s at x is (if approximately steady) given by the equation

$$\frac{d}{dx}\left(\frac{P_1 n_1}{h_1}\right) = -\frac{8}{3} n_1 n_2 s^2 \sqrt{\frac{\pi(h_1+h_2)}{h_1 h_2}} \frac{P_1 P_2}{P_1 + P_2} (a_1 - a_2),$$

where the right-hand side depends on the collisions between the two kinds of gas in the layer, s being the semi-sum of the diameters. From these we obtain

$$\frac{dG_1}{dt} = \left(\frac{3}{16s^2} \frac{P_1 + P_2}{\sqrt{h_1 h_2 (h_1 + h_2)}} \quad \frac{1}{p} + \frac{1}{3n}(n_{2\,1}\mathfrak{C}_1 + n_{1\,2}\mathfrak{C}_1)\right) \frac{d^2 G_1}{dx^2}.$$

In the special case, when the masses and diameters are equal in the two gases, the diffusion-coefficient (the multiplier of $\frac{d^2G_1}{dx^2}$ above) has the value

$$\left(\frac{3}{4}\sqrt{\frac{\pi}{2}} + \frac{1}{3}C_1\right)\frac{\lambda}{0{\cdot}677\sqrt{h}} = \frac{\lambda}{\sqrt{h}} 1{\cdot}8.$$

It is therefore inversely as the density, and directly as the square root of the absolute temperature. And the case of two infinite vessels, connected by a tube of length l and section S, and containing two gases whose particles have equal masses and diameters, the rate of flow of either is $\frac{S\rho\lambda}{l\sqrt{h}}$ 1·8 in *mass* per unit of time.

Other cases are treated; and among these it is shown that with equal masses, and constant semi-sum of diameters, difference of diameters favours diffusion. The remainder of the paper is devoted to the interdiffusion of two gases whose particles have masses in the special ratio 16 : 1, the case of oxygen and hydrogen. The rate of diffusion (in a tube of unit section and of length l, connecting two infinite vessels filled with the gases (the semi-sum s of the diameters being constant) is given by the expression,

$$\frac{A \,.\, P}{\pi l s^2 \sqrt{h}};$$

where A depends, as follows, on the ratio of the diameters :—

Ratio of Diameters.	A.
0 : 1	3·48
1 : 3	3·31
1 : 1	3·46
3 : 1	3·79
1 : 0	4·26

When the masses are unequal it is shown that the temperature must be kept constant to insure a steady state of diffusion.

3. Intimation of an Improvement in Rankine's Formula for Retaining Walls. Given by Professor Armstrong on behalf of Mr Elliott.

Monday, 17th January 1887.

SHERIFF IRVINE, Vice-President, in the Chair.

The following Communications were read:—

1. The Total Rainfall on the Land of the Globe, and its Relation to the Discharge of Rivers. By J. Murray, Esq., Ph.D., *V.P.*

2. Chemical Affinity and Solution. By W. Durham, Esq.

In continuation of my inquiry into the evidence which thermochemistry gives of the truth of my theory of chemical affinity and solution, I would direct attention to the sulphates.

In the first instance, consider the well-known definite compound sulphuric acid H_2SO_4. The heat which is evolved on building up this acid from its elements is 192920 units. When it is dissolved in a large quantity of water the mixture evolves 17850 units, making in all 210770 units. Now, consider how this is made up. First, we have H_2O with a combination heat of 68360 units, then SO_3 with 103240 units. Further, we know that S combines with H_2, evolving 4740 units of heat, and let us assume, according to my theory, that all the affinity of the S for O is not exhausted on the combination SO_3, but that part remains in a less intense form which can act on the O of the water, and in conjunction with H_2 can evolve 34413 units more, this being its average action on the three atoms of O already combined. Add these numbers together, and we have almost exactly the heat of formation and solution of H_2SO_4. Thus—

$[H^2O]$	= 68360	$[H^2, S, O^4]$	= 192920
$[S, O^3]$	= 103240	$[H_2SO_4, Aq]$	= 17850
$[S, H^2]$	= 4740		
$[SO^3, O]$	= 34413		
	210753		210770

Now, of course, I have assumed that S will develop 34413 units in addition after combining with three atoms of O. Let us see, however, how this way of looking at the combination helps us as we go on. Take next a very different sulphate, viz., $BaSO_4$. In building this salt up from its elements, 338070 units of heat are evolved. Analysing this in the same manner as in the case of H_2SO_4 we have $[Ba, O] = 124240$ units of heat, $[S, O^3] = 103240$ units, together equal to 227480 units, leaving 110590 units to be evolved on combination of BaO with SO_3. Whence do these 110590 units come? We have the answer at once when we know that $[Ba, S] =$ about 109600. It is evident the S acts upon the Ba with as much energy as if BaS were actually formed, and this is the cause of the combination. In accordance with my theory, the S cannot act to any extent on the O of the BaO owing to the energy with which the Ba holds the O, being represented by 124240 units instead of 68360 as in the case of H_2O. Thus we have—

$[Ba, O]$	124240	$[Ba, S, O^4]$ 338070
$[S, O^3]$	103240	
	227480	
Difference	110590 = $[Ba, S]$ 109600	
	338070	338070

Further, consider how the heat of neutralisation is accounted for. BaO, on being dissolved in water, evolves 34520 units of heat, SO_3 evolves 39153, and the difference between the sum of these and 110590 is the heat of neutralisation. Thus—

$[BaO, Aq] = 34520$
$[SO^3, Aq] = 39153$
Neutralisation = 36896
110569

Again, take $SrSO_4$, and proceeding exactly in the same way, we have the following result:—

$$
\begin{array}{lrr}
 & [\mathrm{Sr, O}] = 128440 & [\mathrm{Sr, S, O^4}] = 330900 \\
 & [\mathrm{S, O^3}] = 103240 & \\ \cline{2-2}
 & 231680 & \\
\text{Difference} & 99220 = [\mathrm{Sr, S}]\ 99200 & \\ \cline{2-3}
 & 330900 & 330900
\end{array}
$$

$$
\begin{array}{rr}
[\mathrm{SrO, Aq}] = & 29340 \\
[\mathrm{SO^3, Aq}] = & 39153 \\
\text{Neutralisation} = & 30710 \\ \cline{2-2}
 & 99203
\end{array}
$$

Now, both these salts are insoluble in water for the same reason, viz., that the full affinity of the S for the metal is exercised, and nothing is left for the H_2 or O of the water.

Let us take next $CaSO_4$, and we shall find some still more remarkable results. In building this compound up 318370 units of heat are evolved. Tabulated in the same way as in the other cases we have—

$$
\begin{array}{lrr}
 & [\mathrm{Ca, O}] = 130930 & [\mathrm{Ca, S, O^4}]\ 318370 \\
 & [\mathrm{S, O^3}] = 103240 & \\ \cline{2-2}
 & 234170 & \\
\text{Difference} & 84200 & \\ \cline{2-3}
 & 318370 & 318370
\end{array}
$$

Now in this case the difference 84200 is not equal to the heat of [Ca, S], which is 92000. The S therefore is not held with the full strength of its affinity for Ca. There are 7800 units to spare. What becomes of them? Consider the following:—

$$
\begin{array}{rr}
[\mathrm{CaO, Aq}] = & 18330 \\
[\mathrm{SO^3, Aq}] = & 39153 \\
\text{Neutralisation} = & 31440 \\ \cline{2-2}
 & 88623
\end{array}
$$

Now this exceeds 84200 by 4423, which is the heat of solution, viz., 4440, and accounts for so much of the difference; but the remarkable thing now comes in, $CaSO_4$ combines with $2H_2O$, and in doing so evolves 300 units of heat more than the heat of solution, viz., 4740, which is exactly the heat of combination of SH_2. We see, therefore, that owing to the whole affinity of the S for Ca not being exercised in the compound $CaSO_4$, the S can exercise its normal affinity towards H_2 of the water, and as a consequence we have $CaSO_4$ slightly soluble, while the other two analogous salts are insoluble. Further, when this compound $CaSO_42H_2O$ is dissolved in water, the excess of its heat above that of solution appears as a negative quantity, therefore its heat of solution is -300. Now this admirably illustrates the meaning of these negative heats of solution, and also two points to which I drew attention:—First, the lowering of intensity of affinity. We have in this case affinity represented by 300 units of so low a tension that its presence can only be detected when acting on two molecules of water. On a larger quantity it has no effect so far as temperature is concerned. The second point is that every molecule of water exercises affinity on every other molecule, but as the work done and undone must be equal everywhere, there is no change of temperature; but it is entirely different if one or two molecules be bound to another foreign body; the balance is then upset, and the result will be a change of temperature in one direction or the other. There are still about 3000 units of heat to account for. Now, I am not prepared to say exactly where these will be found. According to my theory the affinity of the S is now so reduced in intensity that it cannot make its presence known by evolution of heat in the ordinary way. It may be, however, that here we have the explanation of the facts pointed out in my former papers regarding the precipitation of clay suspended in water, by the addition of a very small quantity of a soluble salt.

Take one more example as extremely interesting for the fresh light it throws on the subject—consider Na_2SO_4. In building this compound up from its elements 328590 units of heat are evolved, and in addition the compound combines with ten molecules of water with an additional evolution of 19220 units. Proceeding as in the other cases we have—

$[Na^2,O]$	$= 99760$	$[Na^2, S, O^4]$	$= 328590$
$[S, O^3]$	$= 103240$	$[Na^2 SO^4, 10H^2O]$	$= 19220$
	203000		
Difference	144810		
	347810		347810

Now, how is this difference of 144810 units accounted for? We have first $[Na^2S] = 88200$, and then we know that Na_2O on being dissolved in water evolves 55500 units of heat, which on my principles are due to its unexhausted affinity for the O of the water; this affinity acts upon the O of the SO_3. Thus we have—

$$\begin{array}{ll} [Na^2S] & = 88200 \\ [Na^2O, O] & = 55500 \\ \hline & \,143700 \end{array}$$

We have also heats of neutralisation and solution as follows:—

$[Na^2O, Aq] = 55500$	$[Na^2, O] = 99760$	$[Na^2, S, O^4] = 328590$
$[SO^3, Aq] = 39170$	$[S, O^3] = 103240$	203000
Neutralisation $= 31378$		
		125590
		Difference 458
126048		126048

Heat of solution $= 460$

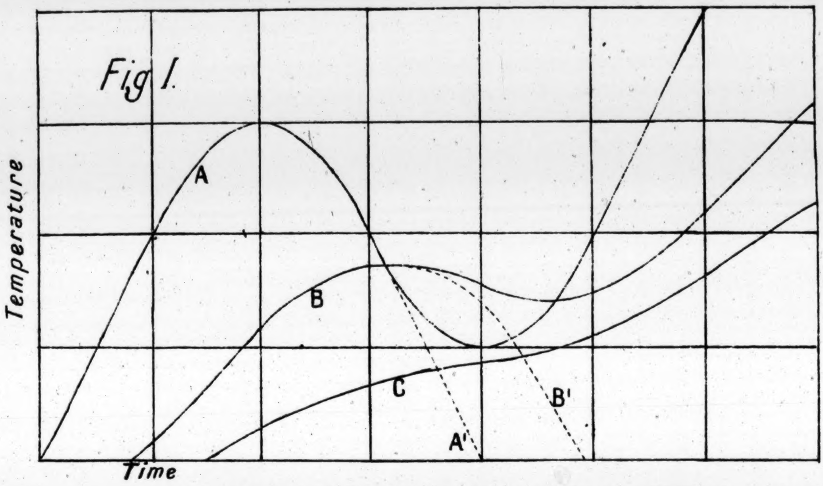
The balance of 18760 units of heat in the formation of the crystalline salt appears as a negative quantity on solution as in the analogous lime salt.

I have made a few determinations of the quantities of some compounds dissolved in water to see how far these conclusions derived from thermo-chemistry are borne out by actual quantities dissolved.

In my last paper I stated that the heats of solution of chlorides varied *directly* as the affinity of the metal for O, and *inversely* as its affinity for Cl. Consider the following table:—

$MCl_2 - MO$, Aq.	Quantity of MCl_2 dissolved in 100 parts of Water.
Ca $= 20560$	63 grains.
Sr $= 26770$	46 "
Ba $= 35980$	35 "

Fig I.



Curves of Temperature 18th Octr. 1886.

Fig. 2.

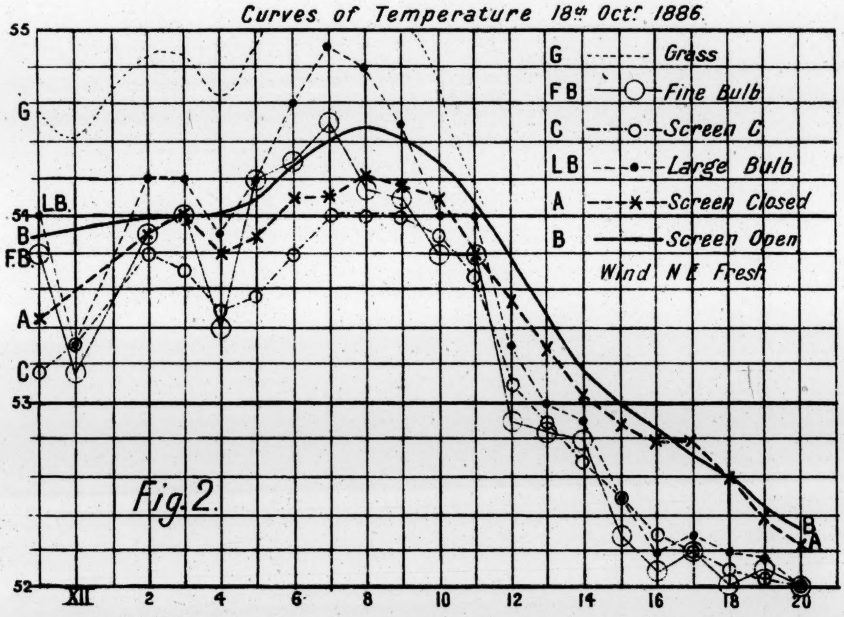
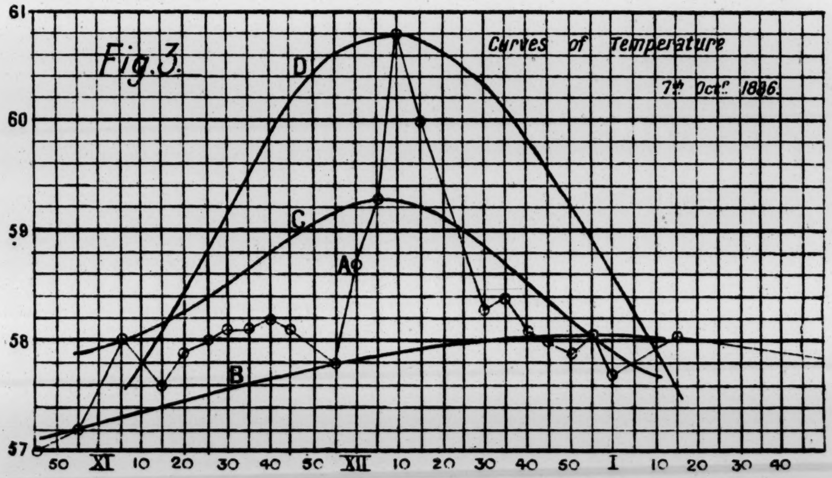
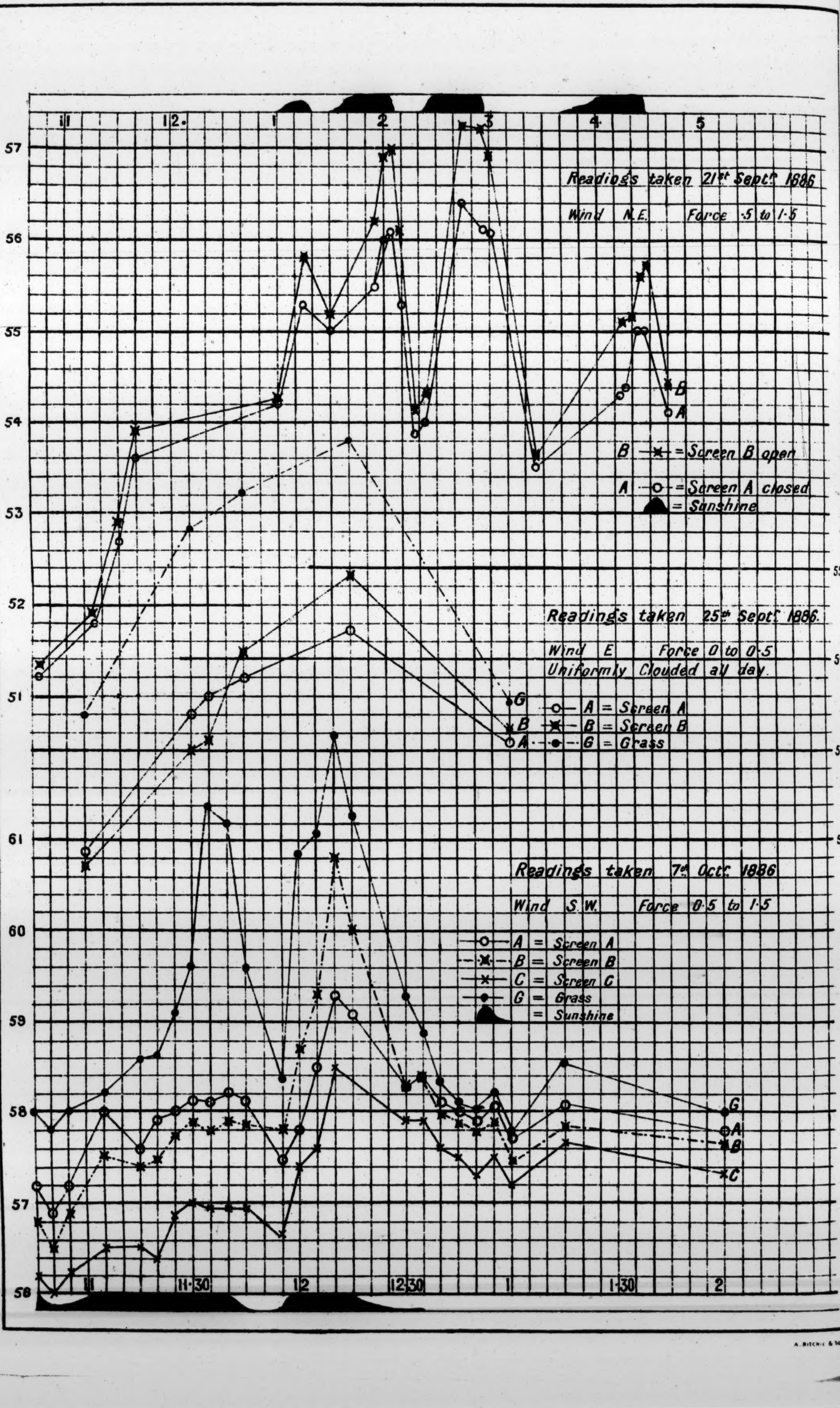


Fig. 3.



A. Ritchie & Son Edin.



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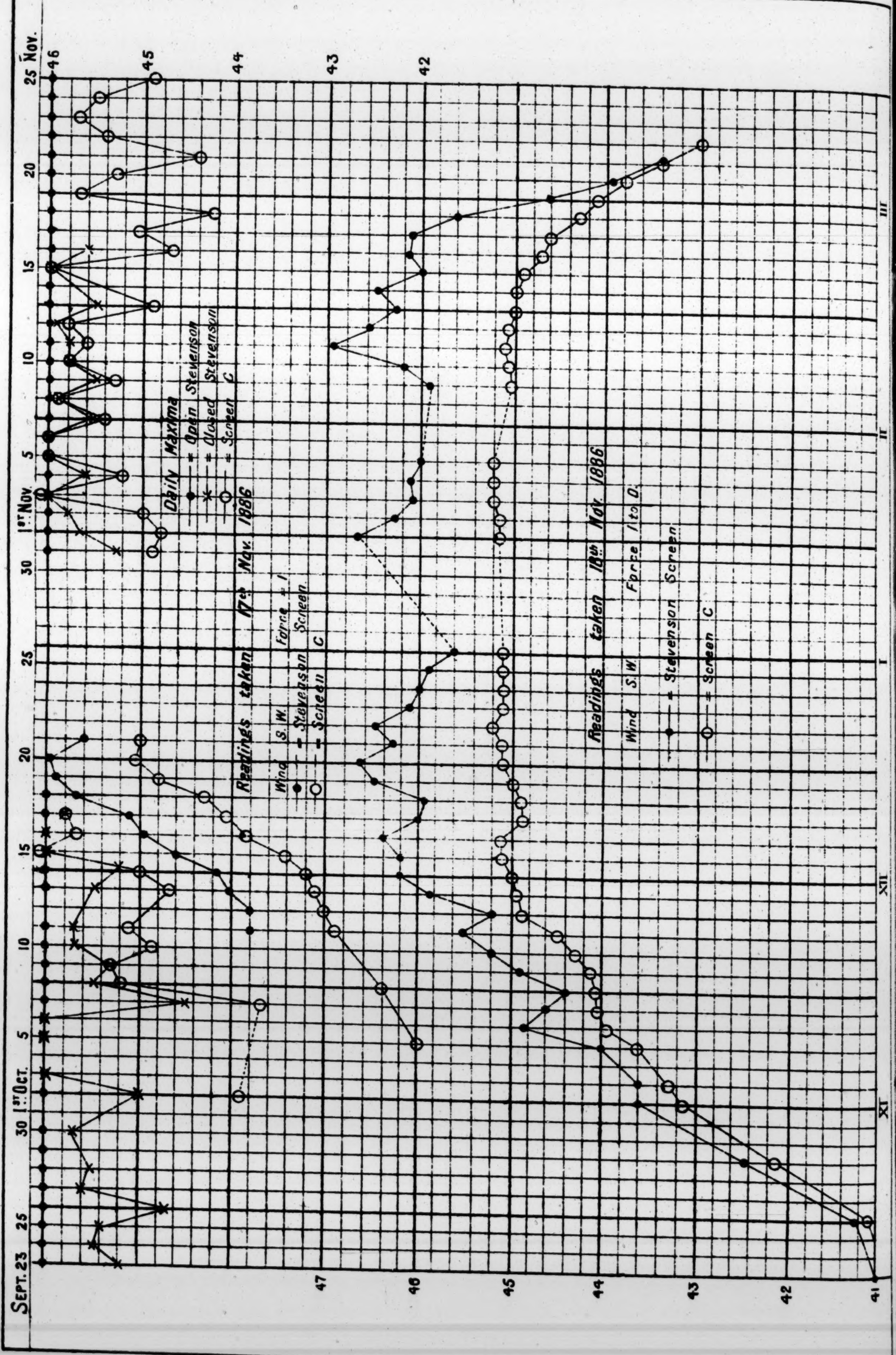

A. Ritchie & Son Edinr

Now, it is apparent at once that the quantity dissolved is almost exactly inversely as the difference of heats, which is in complete accordance with the laws laid down in my former paper. With the nitrates the same relationship cannot be made so clear, as the metals are in combination with O, and the data obtainable are insufficient to trace the various affinities. The results, however, are quite in accordance with the above, although the range is much greater. Thus we have—

Quantity of $M(NO_3)_2$ dissolved.

Ca = 111 grains.
Sr = 50 ,,
Ba = 7 ,,

The differences are in the same proportion as in the chlorides. The sulphates I have already noticed.

The salts of those metals which form insoluble oxides or hydrates I leave for future treatment, as the data obtainable are defective for my purpose. So far as they go, however, they are in complete accordance with those laws I have stated.

These facts seem to me to prove, without doubt, that solution is entirely due to chemical affinity, and that chemical affinity does not act, as has hitherto been supposed, in units, but in all proportions according to the circumstances, and that in chemical combinations of all degrees every atom acts upon every other atom according to its affinity and the position in which it is placed. This way of regarding chemical affinity reduces a perfect chaos of empirical results into an orderly and systematic arrangement.

3. Thermometer Screens. Part IV. By John Aitken, Esq.

(Plates II., III., IV.)

The object of this paper is to describe a new thermometer screen, and to give the results of some trials made this autumn and winter with a Stevenson screen as generally used, and one modified in the way described in a previous part of this investigation.* Also to give comparative readings taken with those screens and with the new one.

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*"Thermometer Screens," *Proc. Roy. Soc. Edin.*, Part 117, p. 661.

Before entering on the subject of the paper, I wish to make a few preliminary remarks on the cause of the difference in the readings given by different screens and by other ways of protecting the thermometers; also to call attention to the interpretation we are entitled to put on curves of temperature drawn from readings taken at longer or shorter intervals of time.

All the methods in use for taking the temperature of the air give different results. One cause of this difference is the more or less perfect way in which the thermometer is protected from the effects of radiation; so that, while all tend to read too high during the day, some read higher than others. But in addition to this, there is another reason why the different arrangements give different results. This second disturbing element we will, for want of a special name, call the *inertia* of the apparatus. By the inertia of the apparatus is simply meant the resistance offered by the thermometer and its surroundings to change of temperature. The inertia may, therefore, be measured by the time taken by the thermometer to acquire the temperature of the air for a given amount of change of temperature. For example, suppose the temperature of the passing air to rise one degree, it would almost instantly heat up any small body, such as a cobweb, to its own temperature, but it would take a much longer time to heat up a larger body, though similarly exposed. The time required will depend on the mass and specific heat of the body, and on the shape and amount of surface it presents to the passing air. We see from this, that if the arrangement of apparatus we use to take the temperature of the air has a small inertia, it may, if the temperature is rising, indicate at first a higher temperature than an arrangement having a greater inertia; and, if the temperature does not remain long enough at its highest point, the apparatus with small inertia will indicate a higher maximum temperature than the other. To illustrate this point, let us first consider a purely imaginary case. We know that the temperature of the air during the forenoon of a summer's day is constantly changing. It does not rise regularly, but rises to a certain extent, then falls, then rises and falls again; and though the general tendency may be upwards, there are many breaks in the curve representing the rise of temperature for the day. This results, as we shall see later on, from the manner in which the air is heated. In Pl. II. fig. 1, the curve A is supposed to represent the changes in temperature of the air, drawn to a scale on which the vertical lines represent half minutes, while the horizontal lines represent half degrees. During one minute the temperature often rises or falls more than one degree. For convenience of illustration, this curve, representing the temperature of the air, is shown as a smooth curve. In reality it is not likely to be so, but in all probability is a very irregular one. Suppose then the curve A represents the temperature of the passing air, then the curve representing the temperature of any very small body, such as a cobweb, will follow this one very closely. But if the body is of any size, then the curve of its temperature will be something like the curve B. Its temperature will rise and fall with that of the passing air, but the two curves will not rise and fall together, because the temperature of the body will go on rising after that of the air has begun to fall, and it will continue to rise so long as the air is the hotter of the two. In the curves, the temperature of B is shown to be rising for more than half a minute after A has attained its maximum, and it is not till A has fallen more than half a degreee, and has the same temperature as B, that the latter ceases to rise, and the curve of its temperature becomes horizontal. After this A and B both fall, but A more quickly than B, and B does not attain its lowest point till after A has passed its lowest, and risen to a certain amount, and acquired the temperature of B, after which both curves rise, but A more quickly than B.

The points to be noted here are: First, that if the top of the curve A had been the maximum for the day, then the inertia of B would have prevented it acquiring the maximum temperature, so that any arrangement of screen having a large inertia will tend to give a lower reading than one with a small inertia.

The second point is, the effect of the inertia in retarding the time of maximum temperature. The curve B does not arrive at its maximum till some time later than the curve A. These considerations help to explain why the daily maximum temperature does not occur about mid-day, when the sun is at its highest, but at a later hour. If we suppose the curve AA′, continued as shown by the dotted lines in the figure, to represent the intensity of solar radiation, then the curve BB′ will represent its heating effect on the air. When the sun is at its highest the air is receiving its maximum heating effect, but owing to its inertia it does not acquire its maximum temperature till a later hour, till near two o'clock, at which hour the amount of heat received is balanced by that lost. After that the temperature of the air falls, but the curve representing its fall is later than A, owing to the inertia of the air, which affects a falling as well as a rising temperature. The same explanation applies to the yearly maximum, and shows why it does not occur in June, when the sun is highest and the greatest number of hours daily above the horizon, but at a later date, when its heating power has considerably diminished.

The curve C in the figure represents the effect which a still greater inertia has on the rise of temperature in a body heated by the air. As the temperature of A never falls quite to that of C, the curve C never falls, but only varies in the rate at which it rises. It will be as well to note here, that all these effects of inertia in checking and retarding the heating of large bodies are quite apart from the question of radiation and its effects on large and small bodies, which, as has been shown in a previous paper, acts in exactly the opposite way, and tends to heat large bodies to a higher temperature than small ones.

Let us now turn to the practical consideration of the subject, and see what the effect of inertia really is on the readings given by different arrangements of apparatus. On Pl. II. fig. 2, are shown curves of temperatures drawn from readings given by different arrangements, each having a different inertia. The curve FB shows the readings of a very fine bulbed thermometer. This thermometer, as was explained in a previous part of this investigation, was constructed to be used as a standard of air temperatures, with which to compare the readings given by the different screens. The bulb of this instrument is 25 mm. long, but it has a diameter of only about 1·5 mm. When in use, it is exposed under a horizontal sunshade in the manner described in Part II., and the bulb is protected from radiation by means of a sheath of pure silver, which fits it closely, but does not press upon it.

The readings given by this instrument were considered to be nearer the true temperature of the air than those given by any other arrangement, as they always kept lowest while there was any radiation. It will, however, be evident that its inertia is very small, and when exposed either without its silver sheath or when covered with chemically deposited silver, its sensitiveness is very remarkable, its indications showing constant fluctuations in the temperature of the air, as the mercury is in a continual state of pulsation whenever there is any radiation effect. These changes amount often to more than a degree in less than a minute, even in an October day, and are much greater in bright summer weather. The fluctuations do not seem to be due to variations in the radiation, as they are observed even when radiation appears to be constant, either under a clear sun or after it has been under a dense cloud for a time. These changes of temperature would seem to be due to the air that is heated on the ground and on other radiation heated bodies not being perfectly mixed with the colder air, one part of the eddy formed by the passing air having more heated air in it than another.*

The curve FB, fig. 2, is drawn from temperature given by this fine-bulbed thermometer without any silver covering. The curve LB shows the readings of another thermometer with a larger bulb, exposed bare, alongside the fine-bulbed one. Its bulb is 22 mm.

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* This conclusion seems to be confirmed by observations made by Mr Dickson with this instrument at the top of Ben Nevis. He informs me that he never observed any of these rapid fluctuations at that station. The air heated on the slopes of the mountain will be carried away sideways by the wind, and the small amount of heating effected by the limited area of ground at the top does not seem to be sufficient to give rise to these changes.

Professor Langley, in his celebrated researches with the bolometer, has observed certain fluctuations in the intensity of the solar radiation from minute to minute. As these fluctuations were ten times the instrumental errors, he is satisfied they have a real existence. He says the solar radiation would have been constant, but that the amount transmitted varied from minute to minute even in what appeared a cloudless sky. It is evident that the fluctuations observed by Professor Langley and those indicated by the fine-bulbed thermometer are not of the same order, although they take place at about the same intervals. The variations given by the thermometer are much too great to be due to variations in the amount of transmitted heat. As we have a satisfactory explanation of the fluctuations indicated by the thermometer, it seems possible that Professor Langley's fluctuations may be due to the same cause. It is very difficult to suppose a want of uniformity in the upper air so great as to cause these fluctuations in such short intervals of time. It seems more probable that the variations observed with the bolometer are due to the imperfectly mixed hot and cold, moist and dry air near the surface of the earth, which will affect the readings of that instrument by absorption and by radiation.

long and 7 mm. in diameter. It has therefore a greater inertia than the other. A and B are the curves of temperature for two Stevenson screens, one with the bottom open and the other with it closed; while C is the curve of temperature given by the new screen presently to be described. The temperatures shown in fig. 2 were taken on the 18th October. There were a few passing clouds at the time, and a strongish north-east wind was blowing. The readings were taken simultaneously by two observers, at intervals of one minute from 11·59 to 12·20, when they were stopped on account of the radiation effect falling to zero, and all the different thermometers reading nearly alike.

It will be seen from an examination of the curves that the fine-bulbed thermometer moved much more rapidly than any of the others. This is not shown so well in the curves as it might be, as the interval of one minute is much too large to show the fluctuation of this instrument, for in the interval between two observations it often indicated temperature much above or below the recorded readings. In the first rise of the curves from 12 to 12.2, there is no very great difference in their steepness. This is probably due to no observations being taken at 12.1; but take the rise beginning at 12.4, and here the effect of the inertia of the different arrangements is very marked. From 12.4 to 12.5 the fine-bulbed thermometer rose 0°·8, and the large bulb 0°·3, while the screens only rose about 0°·1. During the next minute the rate of increase of temperature of the fine bulb greatly diminished, as it was near the temperature of the air, while the rate of the others increased. It will be noticed that the two exposed bulbs FB and LB arrived at their maximum and began to fall before the screens A and B got to their maxima, and so long as the fall continued they kept falling in advance of the others. In the rapid fall which began at 12.11, in one minute the fine bulb fell 0°·9, the large one 0°·7; while the screens A and B fell only about 0°·3, and they took four minutes to fall the 0°·9 lost by the fine bulb in one minute. One effect of this is that while the fine bulb, if read at very short intervals, would give a curve like the edge of a saw, with irregular teeth set at intervals of less than one minute, the inertia of the screens causes them to smooth over these irregularities, and to give a curve with fewer and less abrupt changes.

It will be observed that while these curves show fairly well the effect of inertia on the rate of the heating and cooling of the thermometers, yet they do not show that the arrangement with smallest inertia gives the highest readings, as we concluded from a consideration of the curves in fig. 1. The reason for this is that the curves in fig. 2 are complicated by radiation effects. The large bulb and one Stevenson screen read higher than the fine-bulbed thermometer. This was caused by those arrangements being more affected by radiation than the fine bulb. If we take readings given by the screen C and the fine bulb—the two which are least affected by radiation—it will be observed that the fine bulb rises above and falls below C, somewhat in the manner indicated in the imaginary curves fig. 1, only it rises too high, owing to its being more heated than C by radiation.

It may be as well to note here that the two thermometers exposed under the sunshade did not give more correct temperatures than the screens, but it must be remembered that they were not coated with silver when these readings were taken. It may be interesting to note that the fine bulb gave lower readings than the large bulb, though similarly exposed; this will be seen from an examination of the curves FB and LB. All the curves in this figure follow each other more closely than they would under many conditions. The reason for their comparative closeness on this occasion was doubtless the amount of wind that was blowing at the time the readings were taken. It is evident that a quick circulation of air will have great influence in reducing the time necessary to heat or cool the screens, and will have very much less effect on the exposed bulbs. I much regret I was unable to return to this investigation till late in the season, by which time the radiation effects were greatly reduced, and, owing to the amount of bad weather, very few observations were made. The curves shown in fig. 2 are the result of the only one-minute observations I have been able to make. If these readings had been taken in spring or summer, and when there was sunshine and little wind, the fluctuations would have been much more marked, and the relative steepness of the different curves better brought out.

Turning now to the consideration of the question as to what interpretation we are entitled to put on curves of temperature made from observations taken at longer or shorter intervals of time, a very little consideration will show us that these curves, as generally constructed, are in no sense curves of temperature.

An examination of the one-minute observations given in fig. 2 shows us that the curve of daily temperature is much too complicated to be capable of being represented in the manner it generally is. A few detached observations at hourly intervals tell us really very little about the matter, and to attempt to draw a curve from these can lead to no good, as the curve appears to give definiteness where in fact almost all is unknown; this is particularly the case when there is any radiation. Observations taken at intervals so wide apart as one hour really tell us nothing about the state of matters in the intervals, and yet by connecting these hourly readings by means of curves we not only indicate the temperatures in the interval, but we may even represent the temperature at the hour of observation to be rising or falling when in reality it may be doing quite the opposite.

To illustrate the small value we are entitled to put on curves drawn from hourly observations of temperature, the curves in Pl. II. fig. 3, are drawn from one of the few sets of observations taken recently at regular intervals. The curve A is drawn from observations taken at five-minute intervals, and it will be seen that during the two hours while the observations were made, there were great fluctuations in the temperature. An examination of fig. 2 will, however, show us that even during five-minute intervals considerable changes may have taken place. The curve A, fig. 3, is therefore not so variable as the actual state of the air was when these five-minute readings were taken. Suppose now that, instead of taking the temperature every five minutes, we had done it at hourly intervals. If, for instance, we had selected, not the hour but five minutes to the hour, for the time for taking our readings, then we should have got the curve B. If, however, we had selected five minutes past the hour for our observations, we should have got the curve C, while if the readings had been taken five minutes later the curve would have been D. This latter curve would have shown a higher maximum for the day of 2°·75 above that given by curve B. This difference might have been brought about, as stated, by the hour at which the observations were made, or it might have resulted from the hour at which the high maximum happened to be reached, or it might even have been produced by an error in our time-keeper, either advancing or retarding the time of observations, while keeping them at hourly intervals. It is evident, therefore, that curves of temperature drawn from readings taken at intervals, unless very short, have but little value, and may be most misleading. It would be better therefore, instead of curving the results, simply to connect them with straight lines, so as to enable the eye easily to catch the successive readings, it being understood that these lines give no information as to the state of matters between the observations.

New Screen.

In Part III. of this communication (*Proc. Roy. Soc. Edin.*, No. 121) reference was made to some attempts made to check the entrance of radiant heat through the air passages into the draught tube screen. The most successful results were got by introducing small screens between the bulb of the thermometer and the source of radiation, and so arranging the air circulation that all air that had touched the radiation-heated surfaces was drawn away through side passages, and only the central core of unheated air allowed to pass on to the centre of the screen, and come into contact with the bulb and its surroundings. The plan, however, which was found to work well in a room, in still air, was found to be quite unsuitable for observations in the open air, owing to the wind causing eddies inside the screen which interfered with the proper circulation of the air, and mixed the air heated on the screens with that entering the centre chamber; an attempt was therefore made to see if the principle could not be modified to enable it to be applied in a form suitable for open-air observations.

A number of complicated forms suggested themselves, but as all of them seemed likely to give rise to eddies inside the screen, they were abandoned without trial. At last the simple form shown in fig. 1 was designed, and a screen of this form was constructed at the beginning of October. The figure shows a vertical section through the centre. As will be seen, it is of extremely simple construction. The circulation of air through this screen is entirely affected by the natural movements of the air. No modification of it with draught tube has yet been attempted. The screen consists of two distinct parts,—the lower, or screen proper, surrounding the bulb of the thermometer, is constructed to protect the thermometer from all radiation from below, while the upper part protects the lower screen from the direct rays of the sun. The upper part consists of a square sunshade AA made of wood, and supported in a horizontal position at the proper height from the ground by means of four wooden supports LL attached to the corners, and fixed firmly in the ground. If the direct rays of the sun fell on AA it would get highly heated, and would heat the air on the under side of it, which might affect the readings of the thermometer. To prevent this, another piece of wood MM is placed over AA parallel to it, but with an air space between the two, to check the passage of heat downwards. The lower part of the screen consists of the three plates C, D, and E, fixed parallel to each other and to the lower side of the sunshade AA in the position shown. The plates are held in their places by long screw nails passing through the four corners. These plates may be made of any substance that is a non-conductor of heat; wood is the only material yet tried. The bulb of the thermometer *t* is placed in the space between the two plates C and D, where it is protected from all radiation both from above and below, and to protect it from the horizontal radiation, it is surrounded by the annular piece F shaped in the manner shown. The stem of the thermometer *t* passes upwards through the sunshade, and is protected by means of the louvred box N.

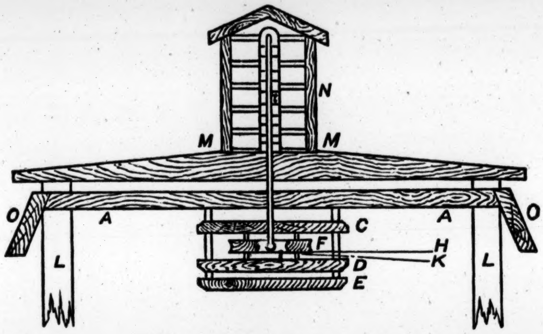

Fig. 1.

When the screen was first used, the sunshade part consisted only of the two pieces AA and MM, but when the sun got very low in winter it was found to affect the correctness of the readings, and the small vertical sunshades OO were added to the screen. These pieces of wood are fixed in a sloping position, to prevent any air heated on them from passing downwards towards the thermometer. As these pieces will slightly interfere with the air circulation, it is possible the screen will act better without them, if observations do not require to be made when the sun is very low. They are not, of course, required on the north side of the screen, or where houses or trees shut out a low sun. The best size for the sunshade has yet to be determined. If made larger than shown, say 3 feet square, then only a very low sun could have any effect on the readings. This screen for future reference is called screen C.

Let us look at the action of this screen when placed in the open air. It will be seen that the air has a very free circulation through it; the plates being horizontal and placed at a distance from each other, the air has a perfectly free passage through it from whatever direction it may blow. It will be further noticed that the bulb is perfectly protected from radiation from all bodies outside. Turning now to the manner in which the heat absorbed by the screening surfaces is prevented from affecting the readings of the thermometer: First, the large sunshade AA prevents any part of the screen proper from being heated by the direct rays of the sun, and it has thus only the diffused radiation to contend with. The under side of AA will be a good deal heated, but the hot air in contact with it will pass between AA and C, and not come into contact with the bulb. The plate E, with the air space between it and D, prevents the heat radiated from the ground passing upwards to the upper surface of D. If D was a perfect non-conductor, E would be unnecessary. The only hot air that really gets into the screen surrounding the bulb is the air heated on the under surface of C and upper surface of D, and also that heated on the upper and under surfaces of F. As will be seen from the construction of the screen, very little heat falls on the surfaces of C and D, so they will be but little heated; but the air that gets heated on these surfaces does not come into contact with the bulb, but tends to flow straight through and out at the other side of the screen, keeping to the surfaces of the plates. This will not be the case when the wind is strong, but when the conditions are trying, that is, with little wind, it seems probable that the heated air will pass through without mixing much with the cold. It will be seen from this that the radiant heat which will affect the thermometer will be that absorbed by the two horizontal surfaces of F, and the heat absorbed at the outer surfaces of F, and conducted through the wood into the inner chamber surrounding the thermometer bulb. The amount of heat received from the first of these sources will be very small, as the horizontal surfaces of F receive most of their heat at second hand, that is, after being radiated and reflected from C and D. The amount received by direct radiation from without is very small, and is represented by the angle KIH in the figure; from which it will be seen that these surfaces have a very limited exposure to outside objects, and the amount conducted through F is probably very small. It will be seen from the sketch that the annular piece F has its outer surface groove-shaped. The object of this groove is to prevent the air heated on it from flowing into the inner chamber, the groove conducting it round the outside. How far this groove is necessary, or to what extent it improves the readings, I cannot say, nor can I say whether the double bottom is necessary, or whether the passage between F and D might not be abolished, and the screen and sunshade thus made smaller. It was thought advisable to take all these precautions, as there was not time this season to work upwards from the simpler to the more complicated; these points were therefore left for future consideration.

It may be mentioned that this screen has been tried without the annular piece F, and it was found to work very well, but did not give quite such low readings as with it in, and its inertia was also much less without the ring. When the piece F is out, the bulb is freely exposed to radiation from all surrounding objects; but as the space between C and D can then be reduced, the bulb does not get a very wide view of the outside. Its readings without the annular piece were very much more correct than the Stevenson screen. This at first may seem strange, as the bulb of the thermometer in it is much more freely exposed to radiation than the one in the Stevenson screen. The reason for its lower readings would appear to be that it is exposed to the radiation from trees and other objects high up, freely exposed to the wind, and therefore cooler; whereas the bulb in the Stevenson screen is exposed to the highly heated grass. This new screen without the ring has not been tried in a situation exposed to walls and other large surfaces that get highly heated in the sunshine.

Owing to the lateness of the season, no trials with this screen have been made under severe test conditions. In the beginning of October it was fitted up on the lawn, and near it was placed a horizontal sunshade under which was placed the fine-bulbed thermometer with its silver sheath, and a considerable number of comparative readings were taken. This was done on a number of days and in different conditions of weather, and the screen has proved itself to be considerably in advance of all the others. Its readings being quite as good as those given by the fine silvered bulb, I shall not attempt to say which gives the lowest readings, as the conditions of the trials have not been sufficiently severe or varied enough to bring out any decided difference; but under all conditions yet tried the screen was quite as low as the silvered bulb. It is unnecessary here to give any detailed account of these trials by themselves, but I shall presently give some comparative readings taken with this screen, with the Stevenson screen with the bottom open, and with it closed, also with the silvered bulb, showing that the new screen gives much lower and more correct readings than either of the other screens, and, as the circulation through it is very free, these lower temperatures cannot be due to the high inertia of the new screen.

Trial of Screens.

In the first and second parts of this communication the results were given of some trials of different methods of protecting the thermometer against the effects of radiation. The conclusions arrived at were, that the most correct readings were given by the thermometer placed in a strong current of air produced by a suction fan, or by a fine-bulbed thermometer exposed under a sunshade with its bulb protected by a silver sheath. Readings taken by these two methods agreed very well with each other, and either of them was taken as a standard of temperature with which to compare the readings given by the different screens. Compared with these standards, the ordinary Stevenson screen was found to give readings of from 1°·3 to 2°·8 too high, when there was much radiation and little wind. Further, it was found that when the Stevenson screen was closed with either a louvred or solid bottom the error was greatly reduced.

After these tests were made, the Scottish Meteorological Society took up the matter, and made a number of comparative trials with the apparatus. The first of these tests were made at Granton in the summer of 1885 by Mr H. N. Dickson. After the Granton work was concluded, Mr Dickson took some of the screens to the top of Ben Nevis, continued the investigation there, and produced a most careful and elaborate set of observations under the conditions existing at the top of the mountain. The Ben Nevis work has not yet been published, but Mr Dickson communicated some of the results of his work at Granton to the Royal Society of Edinburgh.* In this communication he gives curves of the temperatures for two days. These temperatures were taken by the fan apparatus, the silvered bulb, and one Stevenson screen with bottom open and another with it closed. On examining these curves I was much astonished to find that they confirmed none of the conclusions arrived at from trials made here. At Granton the fan and silvered bulb did not give the best results; the silvered bulb read highest throughout the whole of the second day, and further there was little to choose between, in the readings of the Stevenson screens with open or closed bottoms.

These results obtained by Mr Dickson at Granton are so different from mine, that I thought it necessary to reconsider the matter, and again go over my work, under the conditions existing here. In my first trials I had only one Stevenson screen; its readings were compared with those given by the fan apparatus and the thermometer with silvered bulb; and, comparing the readings with the standards when the bottom was out and when it was in, the result was greatly in favour of the observations made with the bottom closed. Mr Dickson made his trials with two screens,—one sent by me with louvred bottom and double top, and another of the ordinary pattern, that is, with bottom open.

In the autumn, my old screen with its louvred bottom was returned from the trials at Granton and Ben Nevis, and I obtained a new one of the standard pattern and exactly similar, only with open bottom and single top; the latter of these screens in the following is called screen A, and the old one B. The screens were fitted upon the lawn at a distance of about 15 feet apart, in as nearly as possible similar exposures to sun and wind. This was done on the 14th September, and in order to make sure that both screens gave the same temperature, comparative trials were made with them, the bottoms of both being open. Though the day was not a very suitable one for the test, as there was not much radiation, still the old screen was found to give higher readings than the other by about half a degree. This was owing to the paint being dirty, and the surface of its louvres being better absorbers of heat than the louvres of the new and clean one. The screens were now painted white, after which they looked nearly alike in whiteness, and on trial with both bottoms open were found to give readings nearly alike.

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* *Proc. Roy. Soc. Edin.*, No. 120, p. 199.

As it would be almost impossible to set up the two screens in positions exactly alike with regard to exposure to wind, radiation, &c., both screens were provided with movable bottoms, so that either could be worked with the bottom closed while the other was open. When testing the screens, sometimes the same screen was kept closed throughout the whole day, at other times first the one then the other would be closed, while the external conditions remained constant, so as to check any difference in temperature due to position or condition of screen, direction of wind, &c.

In these trials the ordinary thermometer used for taking wet and dry bulb observations was employed. The wet bulb with its apparatus was removed, and the thermometer in each screen was placed where the wet one usually is, the index of the instrument being turned slightly round, so that it could be read by opening the door to only a very small extent. The object of this was to prevent radiant heat entering and altering the readings while they were being taken; also to prevent radiation heating the inside of the screen. The thermometers with which most of the observations were made had round bulbs about 8 mm. diameter, but others with smaller bulbs were occasionally used. All the thermometers were graduated on the stems, had wide scales, and were easily read. In addition to the Kew corrections, they were all carefully compared with each other in water at intervals of 5 degrees or less. The room where these comparisons were made was heated to the temperature of the water, so that the temperature of the large volume of water surrounding the thermometers might be kept constant, and the errors due to imperfect mixing be as small as possible. It may be also mentioned that the same thermometers were not kept in the same screens, but they were changed from time to time, to check any error that might arise from any unknown difference in the thermometers, and every precaution that could be thought of was taken to check the results.

The maximum thermometers used were all graduated on the stems and placed vertically in the screens with their bulbs in the usual position. They were held in their places by spring clips, to prevent the position of the index being altered by shaking, a source of error to which the metal-framed instruments loosely hung are very liable. The instruments were carefully tested with their stems vertical. The common maximum thermometers with metal frames and placed horizontally were discarded, as they were found to give most uncertain results, and never agreed with the others in the same screen, owing to the index in these instruments moving too easily, and more or less easily at different parts of the bore, thus forming a longer or shorter air space at different parts of the scale. Further, under certain conditions they gave different readings from the ordinary thermometers with freely exposed bulbs. They read too low if the high temperature remained only a short time, owing to their greater inertia; and in the open Stevenson screen they read too high when radiation remained strong for any length of time, owing to their larger surface causing them to be much more heated than the thermometers with freely exposed bulbs, as the frame and thermometer really act as one surface. The maximum thermometers were changed from screen to screen, generally every day or second day, as an extra check on possible errors.

A great number of observations were made, extending from the 15th of September to the end of November. On many days when there was much radiation, a great number of readings were taken at short intervals, and on as many other days as possible the temperature was taken by maximum thermometers. The result has been entirely to confirm my first conclusions. The closed screen always gave lower readings than the open one.

The fan apparatus was not put on trial, as there were too many readings to be taken without it. In a trial like this the readings of all the instruments ought to be taken at the same moment, owing to the constant changes of temperature in the air. In practice it is therefore desirable to make the number of thermometers to be read as few as possible. The silvered bulb was, however, again put on trial, and as before, it gave readings much below the Stevenson screen; considering the season, its readings were as much below the screen as was observed in the first trials already mentioned.

On 21st September a number of readings were taken from time to time during most of the day with the two Stevenson screens, the bottom of A being closed, while B was open. These readings are marked off at the top of Pl. III., and the different readings connected by straight lines. The day was fine, with passing clouds, a little wind, and radiation fairly strong for the time of the year. It will be observed, that till after mid-day there was but little difference in the readings of the two screens; this was owing to there being but little radiation before that hour. At a little before 12 o'clock the open screen was only 0°·3 above the closed one. A little before 1 o'clock the bottom was taken out of A, and by 1 o'clock both screens read nearly alike.

The bottom was again put in A, and when the next reading was taken at 1.15 the open screen read 0°·5 higher than the closed one, and during the whole day the open one gave the highest readings, the amount varying according to the radiation at the time. The black parts at the top of the diagram represent sunshine; they cannot, however, be very correct, as they are drawn from the notes taken at the hour the readings were made, and thus only represent the condition of matters at that time, no intermediate record being taken. A sunshine recorder would have enabled me to put in these curves more correctly. The general result is, however, very easily seen from the record given. It will be noticed, that whenever there was sunshine the open screen read much higher than the closed one, and that during the absence of sunshine they tended to read alike, but it was not till after radiation had entirely ceased that they read quite alike. At the 2 o'clock and the 2.5 readings the open screen was 0°·9 higher than the other, while a little before 3 o'clock the difference was as much as 1°·1.

While this trial was in progress, in addition to the thermometer placed where the wet bulb usually is, and which gave the readings shown on Pl. III., another thermometer was hung up at the back of each screen, with its bulb in the place where the bulb of the maximum thermometer is usually situated, and readings of these thermometers were taken at the same time as the others. These readings showed that the thermometers placed at the back of the screens indicated on this occasion a greater difference than those placed at the sides. The one placed at the back of the screen with open bottom was as much as 1°·4 higher than the one at the back of the closed screen.

The readings given in the middle of Pl. III., taken on the 25th September, show that the open bottom affects the readings not only on fine sunshiny days, but also on dull ones. On the 25th, the sky was uniformly covered all over with a dense mass of clouds, through which the sun was never visible. But though clouded, there was a good deal of heat reflected and radiated from the sky, the surfaces of all exposed bodies were hotter than the air, and the temperature of the grass rose as much as twelve degrees above the temperature of the atmosphere. At the beginning of this trial screen A was open, while B was closed. At 11 o'clock, when the first readings were taken, there was but little difference in the temperatures given by the two screens, but at 11.35 the open screen read 0°·5 higher than the closed one. After the 11.35 reading, the bottom was taken out of B, and put into A. After which it will be seen that the lines connecting the temperatures cross each other, and B, which at first was closed and read lowest, now that it is open reads higher than the closed one by 0°·6. These readings show that even on a dull day there may be a considerable difference in the readings given by the two screens.

The third set of observations at the foot of Pl. III. show a series made on the 7th October with the two screens A and B and with the new screen C. In these curves, as well as in those taken on the 25th September, there is a curve marked G. This curve represents the temperature of the grass, and was taken by means of a thermometer placed with its bulb on the grass underneath the open screen. This curve is not drawn to the same scale as the others, as there would not be room for it; the temperature rises so high it could not be represented within the limits of the plate. For this curve each space between the lines represents 1 degree, instead of 0°·2 of a degree, and as the disturbance produced by the hot grass will be in proportion to the excess of its temperature above that of the air, the lowest curve—namely C, in the 7th October observations—is taken as the base line, and the excess of the temperature of the grass above C is marked off. For instance, at 11.30, the temperature of the air as given by C was 57°, while the grass was 70°, or 13 degrees above C. Read in this way, the G observations show us that at 12.10 the grass was 18·5 degrees above the temperature of the air.

No curve is given of the silvered-bulb observations, though they were taken at the same time. The reason for this is that they would only confuse the figure, as they were practically the same as those of the screen C, only they were sometimes a little higher and at other times a little lower than C, owing to the smaller inertia of the silvered bulb.

The day on which this trial was made was fine, with some wind from the south-west till near mid-day, when it fell, the air was clear, and there were a few passing clouds. It was not thought advisable to take readings till near 11 o'clock, as the Stevenson screens were quite wet in the morning, and tended to read low, owing to the evaporation. At 10.45, when the readings were begun, the bottom of screen A was open, and B closed. Readings were taken with the screens in this condition till 11.45. After which screen A was closed, and B opened. At 12.30 the condition of the screens was again reversed, B being closed and A open.

It will be noticed that from 10.45 to 11.45 there was not much difference between the open screen A and the closed one B, even though the radiation effect was strong, as will be seen from the amount of sunshine indicated by the black area below the curves and by the curve G. The maximum difference between A and B amounted to only 0°·5; this was probably due to the strongish wind blowing at the time. After 11.45, when the bottom of A was closed, and B opened, the lines connecting the temperatures cross each other, and B now, instead of being the lower, becomes much the higher, the open screen showing a maximum difference of 1°·5 at 12.10. This great error in the readings of the open screen was doubtless due to the wind dying away at this hour. After the bottoms were again reversed, the lines again cross each other; but as the sun did not again come out, and the radiation curve G fell very low, there was not much difference in the two screens; but it will be observed, that as long as the readings were taken, the open one always read a little higher than the closed one.

The curve C shows the temperatures given by the new screen, fig. 1. It will be observed that its readings are very much below those of either of the Stevenson screens. This difference varied from time to time, according to the amount of radiation, but it always remained during the whole time of the observations a good deal lower than the others. At 12.10 it attained its maximum difference, being at that hour 2°·3 lower than the ordinary Stevenson, and 0°·8 lower than the closed Stevenson.

In addition to the observation shown on Pl. III. a number of others have also been taken, but the particulars need not be given here. The general result has always been the same. The Stevenson screen with closed bottom always read lower than the open one, and the C screen lower than either, the amount depending on the radiation, wind, &c., at the time. The observations on Pl. II. fig. 2, show the readings of the three screens on the 18th October. On this occasion the difference in the screens was not great. It will be seen that the open Stevenson only went 0°·5 higher than the C screen, while the closed one was only 0°·2 higher. This small difference was due to the lateness of the season, the small amount of radiation at the time, together with the strength and direction of the wind. The wind was fresh, and from the north-east; it therefore entered by the cold side of the Stevenson screens, and tended to keep the side of the screen on which the sun was shining colder than if it had come from the opposite direction.

Further, in addition to these observations, made at short intervals of time with the ordinary thermometers, the temperature has been taken almost every day in the three screens by maximum thermometers, and the result is always the same. On very few days do they all read alike; only when there is stormy weather and dense masses of clouds; at all other times there are differences, but the three screens always keep the same relative position, C being lowest, and the closed screen lower than the open. It may be mentioned, that in these trials with the maximum thermometers, the same instruments were not always kept in the same screens, but were generally changed every morning, to equalize any instrumental errors, and screens A and B were worked alternately open and closed.

Even so late as the beginning of November, I was astonished to find that the three screens gave very different readings, as will be seen from the following maximum temperatures observed on 31st October and 1st and 2nd November:—

Date.	Stevenson Closed.	Stevenson Open.	Screen C.
October 31	55·4	56·1	55·0
November 1	51·5	51·8	50·6
,, 2	52·8	53·0	52·0

On these three days the weather was very fine, with sunshine and little wind, and on all of these the ordinary Stevenson gave readings of from 1° to 1°·2 higher than the C screen.

These differences in the readings astonished me greatly at the time; and when on the 16th of November a difference of more than a degree was again recorded in the readings of the Stevenson and the C screen, I began to have some doubts as to the correctness of the results, as I thought that long before November arrived radiation would be so weak that it would not interfere seriously with the correctness of the readings given by the Stevenson screen; and yet the observations showed that on some days in this month the error was very considerable. It was, therefore, thought advisable to make some more trials of the screens by means of ordinary thermometers, and taking readings at short intervals. The morning of the 17th November being fine, with little wind and a cloudless sky, readings were taken at five-minute intervals. On this occasion it was not found possible to make a comparison between the ordinary Stevenson screen and the modified form, because, owing to the lowness of the sun, the shadows of the tops of one or two distant trees passed across the screens, sometimes one and sometimes the other being in the shadow; and as the screen in the shade always read lowest, it was impossible to compare the open with the closed one. They were therefore both worked open, and the readings taken of the one in sunshine. These readings, with those given by screen C, are shown in the middle series of observations in Pl. IV. The readings were begun at 11.15 A.M., but the Stevenson screens not being dry, no readings were taken with them till 11.45, at which hour there was a difference of 0°·9 between screen C and the Stevenson. At 12.5 the difference was as much as 1°·2, after which it fell to about 1°·0 at 12.30; after this hour the weather changed, the sky clouded all over, the radiation effect gradually fell, and the readings having no further interest were stopped.

The following day, the 18th, being a most perfect day, the trials were continued. On this occasion the sun rose in a cloudless sky, and shone brightly till it set. The sun was warmer, and the wind less than on the previous day. All the conditions were thus favourable for a trial of this kind, as they tended to bring out in a marked manner any differences due to radiation. As on the previous day, the readings were taken at five-minute intervals, with two short breaks. They were begun at 10.15 A.M., and continued till 3.15 P.M.; the temperatures are all shown in the lowest series of curves in Pl. IV. It will be seen that on this occasion the difference was often more than 1°·0, and attained a maximum of 1°·85 at 2.20 P.M. The wide separation of the curves at this hour was due to the wind dying quite away, so that, though the louvres were not exposed to so strong a radiation at that hour as they had been at an earlier part of the day, yet they got more highly heated, as there was no wind to cool them.

It may be mentioned that the fine silvered bulb was also on trial on these two days. It is, however, impossible to enter its readings on Pl. IV., as they were almost exactly the same as screen C, scarcely ever varying from it more than 0°·1; only once it was observed to vary 0°·2, but generally the two readings were the same. We may, therefore look on the readings given by the C screen as nearly correct on those days, and those given by the Stevenson as too high.

We now come to the consideration of the cause of this very great error in the Stevenson screen, so late in the year, when radiation is so much reduced. As the error is principally due to the heating of the louvres by solar radiation, and as the temperature to which they are raised depends on the amount of heat received by them, and the rate at which this heat is carried away by the air, we shall consider these two points separately. First, as to the radiation. On the two days on which these trials were made, two different kinds of radiation thermometers were exposed to the sunshine. One of them was an ordinary vacuum black-bulb thermometer, and the other was one of my radiation thermometers having a plate 14 inches square. The following are the maximum readings recorded:—

Date.	Vacuum Thermometer.	14″ Black Plate.
November 17	81°·5	96°·5
,, 18	91°	113°·5

These radiation temperatures seem high for so low an elevation as the sun attained at that date, for we must remember that the temperature of the air was low at the time. Suppose it had been a warm summer day, and the temperature of the air 75°, then the same solar radiation as that of the 18th would have raised the temperature of the vacuum thermometer to 121°, and the other one to 143°·5. Still stronger radiation effects were observed on the 2nd December; the air on this day was 32°, vac. 81°, and black surface 111°. This seems to point to a wonderful diathermancy of the air in winter compared to summer. I write without sufficient observations, but I imagine an equally low sun in summer would not have anything like this heating effect.* Then again, the louvres of the Stevenson screen are exposed almost perpendicularly to the rays of a low sun; they therefore receive more heat than from an equally hot but higher sun in summer.

As stated, owing to shadows passing over the screen, it was not found possible to make a comparison during the winter months between the ordinary Stevenson and the modified form. This, however, does not seem to be a matter of much importance, as we can scarcely expect to find much difference in their readings at this season. The daily maximum readings up to the 16th November do indicate an advantage in favour of the closed form; but this may possibly be the result of accident, and due to the open screen being more frequently in sunshine than the other, at the time the maximum temperature was attained. The sun at this season being very low, its rays strike so nearly horizontally they scarcely heat the ground, all their heat being received by trees and other vertical surfaces. There will therefore be but little heat radiated into the screen from the ground, and there will not be much reflected. While the observations were being made on the 17th and 18th November, no thermometer was placed on the grass, as its indications would have been valueless, owing to its showing a great difference in temperature according to the situation of the bulb. It would have read high and above the temperature of the air if it happened to be in sunshine, but if shaded by the grass it would have read much below the temperature of the air, as it was observed that the small hollows in the grass which had got frozen during the night remained frozen all day. It is evident from this that a thermometer placed on the grass would not enable us to say whether an excess or deficiency of heat was radiated in through the open bottom of the screen; but of course some heat would be reflected inwards, however cold the grass.

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* The greater diathermancy of the air in winter than in summer has been observed by M. Soret at Geneva, and Professor Langley has carefully measured it. He finds the greater transparency in winter to be chiefly for rays of short wave lengths, and he finds a close relation between the diathermancy and the amount of vapour in the air. I much regret I have no low-sun summer observations with which to compare my winter ones, but the difference in this climate seems to be very much greater than that observed by M. Soret or Professor Langley. This difference will probably be due to the position of my observations being much further north than Geneva or Mount Whitney, and subject to greater extremes of dryness and moisture.

We may here refer to a point of some interest observed when making these trials in November. In the previous parts of this communication frequent mention has been made of the quick fluctuations in the temperature of the passing air, as revealed by the constant pulsations of the fine-bulbed thermometer. It was observed in these last trials that these quick changes were almost entirely absent. The mercury in the fine-bulbed thermometer rose and fell nearly as steadily as the one in the C screen, seldom varying from it more than 0°·1. The apparent dulness in the movements of this instrument would seem to be caused by the entire change in the manner in which the air is heated by solar radiation in winter compared to summer. When the sun is low the ground is but little heated by its rays, and there is no layer of hot air near its surface swept along by the wind, and imperfectly mixed with the colder air above, to give rise to a succession of warm and cold parts; and the heating received by vertical surfaces is distributed through a much greater depth of atmosphere, and is more easily and perfectly mixed with the passing air. The air passing the thermometer has therefore a much more uniform temperature with a low than with a high sun. If this is the correct explanation, then it confirms our conclusion that the fluctuations observed in this thermometer in summer were due to imperfectly mixed hot and cold air, and not to variations in the radiation.

We now come to the consideration of the second point, namely, the power of the passing air to check the effects of radiation. It is very evident that this will depend on the rate at which the air passes over the radiation-heated surfaces. Now, in summer, we have—apart altogether from what we call wind—an unstable condition of the atmosphere which acts quite locally. Owing to the heating of the ground, the air over it is rarefied and tends to rise; its stability is thus constantly disturbed by slight movements. But in winter we have a totally different condition of matters. Radiation from the earth being now in excess of that towards it, the surface of the ground gets cooled, and the air on it tends to become denser, and so keep closer to the ground, and if the country is flat, there is no tendency as in summer to local movements, but on the contrary, the tendency is towards stability. We thus see that the solar radiation effect in summer is to cause—in addition to winds proper—slight local airs, which prevent the surface of the louvres in the screen from being highly heated; whereas in winter the tendency owing to the lowness of the sun is the other way, and we have degrees of calmness in winter, quite unknown and impossible in summer. With these two things, namely, the high heating power of even a low winter sun, and the great calmness of air possible and frequent at this season, we seem to have the explanation of the somewhat unexpectedly great error of the Stevenson screen observed during the winter season. The error at this season is due principally to the heating of the louvres, and but little to heat radiated in through the open bottom.

In connection with the more perfect calms which take place in winter than in summer, and as showing their effects on the comparative cooling produced by them at the different seasons, we may here refer to the readings given by the two forms of radiation thermometers used in this investigation, namely, the ordinary black-bulb thermometer *in vacuo*, and the flat black surface thermometer having an area of 14 inches square. The ordinary black bulb gives a very complicated result, indeed it is extremely difficult to interpret its readings. It measures, however, chiefly the intensity of the solar radiation that is received at the surface of the earth, as modified by the surroundings of the bulb. The flat black surface, on the other hand, gives the intensity of the solar radiation as modified by air currents; the stronger the wind the lower the temperature recorded for a given intensity of radiation; whereas the black bulb *in vacuo* is not much affected by wind, though it is affected by other and rather obscure influences; the flat black surface therefore gives a better indication of the climate than the black bulb. In fine summer weather it was found that the flat black surface generally read about 12 per cent. above the black bulb; but in winter this difference has been found to be very much greater, on account of the more perfect calms at that season permitting the exposed surface to be more highly heated.

But further, it is found that there is a close relation between the difference in the readings of the two radiation thermometers and the errors of the Stevenson screen. An examination of the readings taken in December show that as the ratio of the temperature of the black surface to that of the black bulb increased, the error in the screen increased along with it. When the ratio of the black surface to the vacuum black bulb was under 1·5, the error of the Stevenson screen was about 1 degree, and as the ratio increased, the error increased along with it. This error attained its maximum, as far as was observed, about mid-day on the 13th of the month; at that hour the temperature of the air was 34°, the vacuum black bulb 64°, and the flat black surface 93°. The black bulb was thus 30° above the temperature of the air, while the black surface was 59°, or in the ratio of nearly two. At that hour the Stevenson screen reached its maximum error for the day, being 2°·9 above the standard. It may be noted that when the error of the screen was at its maximum the vacuum thermometer did not register the maximum temperature for the month; indeed it was one of the lowest on the bright days, but owing to the entire absence of wind at the time, the difference in the heating effect of the sun's rays on the two forms of radiation thermometers, and the error of the Stevenson screen attained their maximum.

While on this subject, it may be as well to consider an effect of these two tendencies of air when heated and when cooled,—the one to instability and currents, the other to stability and calmness. Given a condition of weather in which there is no wind, then in summer the effect of heating the air on the ground is to cause ascending currents of hot air. There is thus an influence at work in summer tending to keep the air near the ground at nearly the same temperature at all places within a considerable distance; while the effect of cooling the air in winter is precisely the opposite, as the cooled air tends to sink down into hollows and remain there, where its temperature is further reduced by contact with radiating surfaces. The cooling of the air in winter, therefore, in the absence of winds, will tend to give rise to differences of temperature, which may amount to some degrees within a limited area.

From the above we can see that on a day on which there is no wind, the temperature of the air in an exposed situation, and where the sun is shining, may be some degrees warmer than the air in a cup-shaped hollow, into which the sun's rays do not penetrate, provided always that the sky overhead is clear and radiation into space strong. Take the case of the 18th November: on that day the temperature of the air was 45°, while the grass that was not exposed to the sunshine was under 32°. We can easily imagine conditions in which the air resting on grass at 32° in a hollow into which the sun does not shine might easily be cooled some degrees below the temperature of the air above. These observations are suggested by the peculiar condition of matters reported from different parts of London on the 24th November. In one part of London the maximum temperature was 40°, while at another it did not rise above 32°, a difference of 8°, which might be accounted for in the way above stated, as there was no wind at the time.

A record having been kept for some time of the maximum temperatures recorded by the three screens, the readings taken from the 23rd September to the 25th November are marked off at the top of Pl. IV. The temperatures were not taken every day, but every observation taken is marked. Most of these are from the readings of the maximum thermometers, but on those days on which the screens were under trial, the readings taken at short intervals were used, and the maximum recorded readings selected. As it was impossible within the limits of the plate to record the actual temperatures, only the differences between the readings are shown. The readings of the open Stevenson screen are used as a base line, and the difference between its readings and those of the closed Stevenson and the screen C are marked off, each series being connected by straight lines. It will be seen that on only two occasions did the screen C give the highest readings, and on these exceptional occasions it was only 0°·1 higher. As the days on which these occurred were dull and windy, the differences were probably errors of observation, such as are quite to be expected. It will be observed that the close screen generally held an intermediate position between the open one and screen C, its readings being better than the former, but not so good as the latter.

Accepting screen C as our standard, which we may do for the present, its readings being the same as the fine silvered bulb, an examination of the maximum temperature curves shows us that in autumn and winter the Stevenson screen is frequently more than 1° too high. In the first half of October it was more than 2°·0 too high on two occasions, and in November it was a degree or more wrong eight times—in addition to the seven times shown on plate there was an error of 1°·1 on the last day of the month—and on one occasion in that month it was as much as 1°·75 too high. The differences shown in Pl. IV. are the differences in the maximum readings, but these are not necessarily the greatest differences for the day. Of course, the differences in the maximum readings are what are practically required, yet it may be interesting to note that this may not be the greatest difference for the day. For instance, on the 21st November the maximum readings for the day were 39°·6 for screen C, and 41°·2 for the open Stevenson, thus giving a difference in the maximum for the day of 1°·6 as shown. The day being very trying, a reading was taken at 1 o'clock, when the index of the maximum thermometer in screen C was at 38° and in the Stevenson at 40°·6, or a difference of 2°·6. This great difference was due to there being a dead calm a short time before.

The weather on the different days is not entered on the plate, as it is quite unnecessary; the curves speak for themselves. Whenever the reading of the screen C was much below the others, the weather was fine, and it was only during cloudy and stormy weather that all three agreed.

The observations for December show that the maximum given by the Stevenson screen on eight days was one degree or more above the maximum given by screen C. The maximum difference for the month was 1°·75. The screens gave the same maximum on eleven days, while the mean maximum temperature for the month was 36°·78 by the Stevenson, and 36°·25 by the C screen; that is, the C screen gave an average maximum temperature of fully half a degree below the Stevenson. Of course, the average error is determined very much by the number of bright days in the month. Taking the average error for the fine days of the month, it was about 1°·4, and that would have been the error if the month had been bright throughout. The difference for January 1887 promises to be very small. Owing to the dull and clouded weather, the screens have read exactly alike on almost every day of this month. The observations for December and January are not entered in the plates.

Returning now to the consideration of why the result got by the different screens at Granton differ so much from those obtained here, I think I have taken every precaution to ensure the correctness of my results; and yet we find, even so late as the middle of November, that the Stevenson screen with open bottom gave higher readings than the closed one; also that the thermometer with its bulb sheathed in silver gave, as in the previous trials, readings much lower than either of them, and yet the observations made at Granton show no such differences; how then are we to account for the difference in the results obtained at the two places?

The first thing that suggests itself as a possible cause of the differences is the condition of the louvres in the two screens. Was the one dirtier than the other, or were the absorbing powers of the paints on the two screens different? We have seen that, on a not very trying day, in September, the absorbing powers of the two screens used in my last trials caused a difference of about 0°·5. On a bright day, such as those on which the Granton trials were made, this difference would be greater; and if the closed screen was in the Granton trials the better absorber of the two, this might have neutralised any advantage arising from its being closed. With regard to the explanation of the high readings given by the silvered bulb at Granton, I have great difficulty. No doubt, any imperfection in the cleanness of the silver would increase its absorbing powers and so raise the temperature, but doubtless care would be taken to keep the polish in as perfect a condition as possible.

In addition, however, to these possible sources of error, there is in the situation of the two places where the observations were made an essential difference which would affect the results. The site of the screens at Granton was very freely exposed to every breath of wind, being on a knoll in the middle of a field near the sea shore, perfectly open to the west, north, and east, while the land rose a little to the south, and the screens were at a great distance from trees or anything that could obstruct the free circulation of the air. The site on which the screens are placed here is very different, and not so good in many respects, though I think it will compare favourably with the site of many screens in daily use. Here the screens are on a lawn, and surrounded at no very great distance by trees. As the surroundings of the screens seem to be a matter of greater importance than might at first be thought, it will be as well that I state more fully the conditions surrounding the screens here. Standing at the screens, the view in the different directions is closed in principally by trees. The view to the south and round by west to north-west is closed in by a narrow line of trees running north and south, at a distance of 26 yards at the point where it comes nearest to the screens. From the north-west to the north-east, at a distance of about 24 yards, there is a holly hedge 8 feet high, and also a few trees. From north-east to east are trees at a distance. From east to south-east a holly hedge running north and south, and coming at its north end to within 12 yards of the screens. From south-east to south are stables and kennels at a distance of 35 yards. The ground slopes slightly down to the north. The screens are placed east and west of each other, and all the ground in view is under grass. It will be seen that, while the position is somewhat sheltered from winds blowing from east to south-east, it is fairly open to winds from south to south-west, as well as from north-west and north-east. But, on the whole, it is evident the site is much more confined than the Granton one.

An evident result of the opener exposure of the Granton site is, that the Stevenson screens would be kept cooler there, on account of the freer circulation of the air through the louvres, the screens would thus be both more nearly correct, and therefore nearly agree with each other and with the silvered bulb, at the more exposed Granton site than at the more confined one here. Further, on both days on which the Granton trials were made, the wind entered the screens from the cold side. This of itself is a most important point, especially if the sky is clear, as the cold sides of the screens may be below the temperature of the air, and the passage of the air in that direction prevents the advance inwards of the heat from the sun-heated louvres. I have noticed in the trials here that on all occasions on which the wind was north of east or west the errors were comparatively small. It therefore seems possible that the more open exposure of the Granton site, together with the direction of the wind at the time the two sets of the Granton observations were made, are the principal causes of the difference in our results.

Since this paper was given in, Mr Dickson, who has continued his trials with the Stevenson screens at the top of Ben Nevis, has very kindly furnished me with an abstract of the result obtained in that situation. He says the readings for thirty-four days gave the following mean maxima :—

Stevenson,	Open bottom,	41°·45.
„	Closed „	41°·00.

Thus showing a difference in favour of the closed bottom of nearly half a degree, a result confirming the conclusion arrived at in this and a preceding paper.

The site here being surrounded by trees in almost every direction, may in part explain the reason why screen C, even when the annular piece F was removed, gave such correct readings. We have seen that on the 7th October the temperature of the grass rose 18° above the temperature of the air. Now that is the temperature to which the thermometers in the Stevenson screen with open bottom are exposed, while screen C, by its construction, cuts off all this radiation and exposes the bulb to the radiation from the trees, which will never be so highly heated as the grass, as they have a freer circulation of air through them. This, combined with the very free circulation of air through the screen, and the method adopted for preventing the heated air touching the thermometer, would seem to account for the low readings given by this screen even when the piece F is removed.

There would appear to be an advantage in favour of the C screen, which may be referred to here. We have seen that when the louvres of the Stevenson screen get dirty, they absorb more radiant heat, and so increase the error of the readings. Many screens in daily use must, from this cause, give too high readings. The screen C is, however, not much affected from this cause; indeed, I am not quite certain but that the screen will act quite as well if certain parts are black. For instance, the sides of the plates C and D which are exposed to the bulb of the thermometer might perhaps with advantage be blackened. I have not yet been able experimentally to determine this point, but many of my observations have been taken with a large black patch in the centre of each plate. My reason for testing this was, that if these surfaces are white, they will reflect to the bulb some of the heat which falls on them; but if they are black, they will absorb this heat; and it seemed possible that the increased amount of heat *radiated* by the blackened surfaces, together with the greater amount to which the air in contact with them is heated, might affect the thermometer less than the heat *reflected* by the white. It was not found possible to settle this point by readings taken with the screen under the two conditions and comparing them with the silvered bulb, as the inertia of the two arrangements is so different, and the effect sought for very small. This, with many other matters connected with this screen, will be best settled by means of two screens similar in all points save the one we wish to test. For these trials, however, we must wait another warm season. Although this new screen has acted very satisfactorily up to the present time, giving readings almost exactly the same as the fine silvered bulb standard, and much below those of the Stevenson screen, yet it would be rash to conclude that it will be superior under all conditions of climate. The various influences to which thermometer screens are exposed are so numerous that the unexpected has many opportunities of happening and upsetting our hopes and expectations.

[A Postscript to this Paper will be found immediately after the *Proceedings* of July 1887.]

4. On the General Effects of Molecular Attraction of Small Range on the Behaviour of a Group of Smooth Impinging Spheres. By Professor Tait.

(*Abstract.*)

The present instalment traces some of the consequences of assuming the hard spherical particles of a gas to exert intense molecular forces when at distances comparable with their diameters. The effect of the new term in the virial in counteracting and at last obliterating that due to the impacts, is traced as the gas is gradually compressed.

Next, the spheres (still supposed to attract one another) are regarded as capable of absorbing energy in a vibratory form, and of losing it directly by radiation. In such a case the relative translatory energy may be so reduced that pairs of spheres may *remain* within molecular distance from one another. The bearing of these results upon condensation, dissociation, &c., is given.

PRIVATE BUSINESS.

Mr Nanabhay A. F. Moos, L.C.E., B.Sc., Assistant Professor of Engineering, College of Science, Bombay, was balloted for, and declared duly elected a Fellow of the Society.

Monday, 31*st January* 1887.

JOHN MURRAY, Ph.D., Vice-President, in the Chair.

The following Communications were read:—

1. On a New Formula for the Pressure of Earth against a Retaining Wall. By A. C. Elliott, B.Sc., C.E. Communicated by Professor Armstrong.

There are two main distinct methods of attacking the problem of the retaining wall. The first in chronological order is due to Coulomb, and is variously named, perhaps most commonly as the method of the Wedge of Least Resistance. Briefly characterised, it might be said to depend upon the mathematical artifice of finding the resultant force due to the mutual action of the earth mass, and the wall a maximum, the earth being supposed to yield incipiently under the action of its weight, and in opposition to friction and the reaction in question, along an inclined plane determined so as to fulfil that imposed condition. Coulomb's method has been developed by various writers, and may be regarded as complete.

The second method is due to Rankine. It is based upon two general dynamical principles, both of which are really involved in Coulomb's treatment, but which are there drawn upon as it were incidentally rather than appealed to as fundamental principles. Rankine's first principle is merely a statement that the well-known propositions in regard to the laws of static friction apply in the interior of a granular mass of earth; and, in particular, that there is a coefficient of friction for earth upon earth of any given kind. That some sort of physical datum of this nature with respect to any given kind of earth may be properly assumed does not admit of question; but how far it answers to an ordinary physical constant, or even an ordinary coefficient of friction, is by no means certain. However, objections of this kind apply to Coulomb's method with even greater force, and the author proposes to attempt to push Rankine's theory farther on its present bases, rather than to discuss preliminary difficulties. If, therefore, he shall be fortunate enough to arrive by a path not altogether mistaken at certain results, he would merely say that such are the consequences of adopting these fundamental principles.

The first of the principles just referred to enabled Rankine to formulate the conditions of equilibrium in the interior of an earth mass generally, and in terms of certain data for particular cases occurring in practice.

When he comes to deal with the action of the earth on a wall, Rankine refers to his second principle, which was first distinctly laid down by the late Canon Moseley. Briefly it is merely this:—When a system is in equilibrium under a set of forces, those which are called into existence by the action of the others are the least possible consistent with the given conditions; or, among a set of forces, active and passive, in equilibrium, the passive forces are the least possible.

Rankine uses the term granular mass to indicate that the earth is assumed to have no tenacity or cohesion. Some kinds of material might fairly claim to be so described, but in ordinary practice it is quite common to meet with earth which will stand with a vertical face for a considerable time; but since the action of time and weather will inevitably result in the material ultimately assuming a slope more or less constant for that particular kind, it is not only prudent but necessary to allow for the almost total failure of tenacity or cohesive strength with lapse of time. In the retaining wall problem the effect of assuming any degree of tenacity being clearly operative in reducing the resultant force representing the mutual action of the earth mass and the wall, Rankine makes no scruple to discarding tenacity altogether.

It may be remarked that Coulomb's method implicitly takes account of some degree of tenacity. Nearly all direct experiments have shown considerable divergence between the actual overturning moment of the earth pressure on the wall, and that calculated by Coulomb's or Rankine's method (employing the accepted methods for determining the principal constant), the divergence very commonly amounting to upwards of 50 per cent. in excess of the observed value. The discrepancy is, in the author's opinion, in great part clearly due to the ignoring in the mathematical investigation of the effect of tenacity. On the ground that no satisfactory allowance could be made on account of a quantity which is at once a function of time and weather, Rankine, as has been already remarked, expressly rejects the tenacity from consideration, so that, in the case of Rankine's formula at least, one ought not to be surprised if the calculated should exceed the actual overturning moment of the earth pressure to a considerable extent.

But granting this, or at any rate taking leave of it, there still remains the experimental evidence that Coulomb's method in its complete form, though much more unsatisfactory from a physical point of view, gives better results than Rankine's. Attention has consequently been redirected to Rankine's method with the object if possible of removing this anomaly; and, accordingly, it has been pointed out that Rankine simply applies the conditions of equilibrium, obtaining at a point in the interior of the earth mass, to a point situated in the surface of separation, between the wall and the earth mass, thus tacitly neglecting the boundary conditions. Rankine, in short, neglects the friction between the earth mass and the wall, or supposes the wall to be perfectly smooth. Dr Maurice Levy and Professor Boussinesq have worked at the problem thus presented, but the author is only partially acquainted with Prof. Boussinesq's results, and not at all with Dr Levy's. In 1881 Mr Benjamin Baker read a paper before the Institution of Civil Engineers, "On the Actual Pressure of Earthwork"—a valuable contribution to the literature of the subject, to which reference will afterwards be made. An interesting communication from Professor Boussinesq is printed with the correspondence appended to the paper, which will be found in vol. lxv. of the *Proceedings.*

The author will confine himself to the case where the surface of the earth to be retained is level. Rankine's hypothesis of a granular material and the fundamental principles before mentioned are assumed.

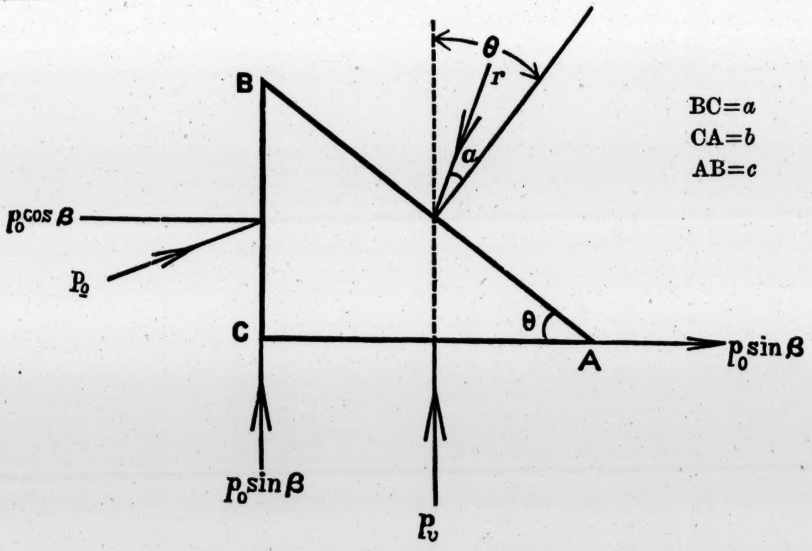


FIG. 1.—Vertical Section of a Right Prism of Earth.

Let ABC be a vertical section of a right prism of earth, whose length-axis is parallel to the inner face of the wall, which will here be assumed to be vertical. Let BC be in contact with the wall, and AC horizontal; also, let the prism be of unit length, and let the transverse dimensions be very small.

Consider the forces acting on the prism :—

(1) Its weight: this may be neglected in comparison with the other impressed forces, which are small quantities of the second order, while the weight is a small quantity of the third order.

(2) The forces acting on the end faces of the right prism :—these are independently balanced as regards the wall, and may henceforth be left out of consideration.

(3) The remaining impressed forces acting parallel to the plane of the section: of these let p_v be the vertical pressure in the neighbourhood of the prism, due to the column of earth; p_0 the inclined pressure exerted by the wall.

β the angle between the direction of p_0 and the normal to the inner face of the wall, drawn outwards. This angle β, in the case where motion is just about to take place along the interface BC, will be equal to the angle of friction for the earth on the wall.

r the pressure of the contiguous earth on the face AB of the prism;

α the obliquity of r; or the angle which the direction of the stress r makes with the normal to AB;

θ the angle BAC.

Considering first the equation of moments, it will appear that the tangential stress on AC must be equal to the tangential stress on BC; but the tangential stress on BC is $p_0 \sin\beta$, which is therefore the value of the tangential stress on AC.

Resolving vertically and horizontally

$$p_v b + p_0 \sin\beta \,.\, a - r\cos(\theta - \alpha)\,.\,c = 0 \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$p_0 \cos\beta \,.\, a + p_0 \sin\beta \,.\, b - r\sin(\theta - \alpha)\,.\,c = 0 \quad . \quad . \quad . \quad (2)$$

Dividing out by c and substituting in terms of θ,

$$p_v \cos\theta + p_0 \sin\beta \sin\theta = r\cos(\theta - \alpha) \quad . \quad . \quad . \quad . \quad (3)$$

$$p_0 \cos\beta \sin\theta + p_0 \sin\beta \cos\theta = r\sin(\theta - \alpha) \quad . \quad . \quad . \quad . \quad (4)$$

Eliminating r, and writing s for the ratio p_0/p_v

$$\frac{1 + s\sin\beta\tan\theta}{s(\cos\beta + \sin\beta\cot\theta)} = \frac{1 + \tan\alpha\tan\theta}{1 - \tan\alpha\cot\theta} \quad . \quad . \quad . \quad . \quad (5)$$

If now θ be regarded as the independent, and α as the dependent variable, to find the condition for α a maximum (5) must be differentiated with respect to θ and $\frac{da}{d\theta}$ put equal to zero in the result —*i.e.* (5) must be differentiated partially with respect to θ. There then results the condition

$$\tan a = \frac{s \sin \beta}{s \cos \beta \sin^2 \theta - \cos^2 \theta} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

In strictness, this step ought to be immediately justified by an examination of the sign of $\frac{d^2a}{d\theta^2}$, under the condition (6). This will be found to involve a considerable amount of labour; and, since the operation is essentially of the nature of a verification, the author proposes to accept, for the present, (6) as the condition for a a maximum, and afterwards to justify whatever assumption this may involve, by a process of verification. The mere fact, however, of obtaining a result which does not necessarily imply that a must be zero, indicates the existence of a maximum or minimum condition different from $a=0$; which is, of course, the condition which a fulfils when it is a minimum as regards mere numerical magnitude. Farther, from the theory of stress, it is known that, corresponding to the maximum value of a, there is a maximum value of equal numerical amount but of opposite sign.

Now the maximum value of a is to be ϕ, the angle of repose; the angle, that is, whose tangent is the coefficient of friction of earth upon earth, for earth of the given kind.

Let θ_1 be the value of θ, when $a=\phi$. Then $\theta=\theta_1$ and $a=\phi$ must satisfy (5) and (6) simultaneously. Therefore—

$$\frac{1 + s \sin \beta \tan \theta_1}{s(\cos \beta + \sin \beta \cot \theta_1)} = \frac{1 + \tan \phi \tan \theta_1}{1 - \tan \phi \cot \theta_1} \quad . \quad . \quad . \quad . \quad (7)$$

and

$$\tan \phi = \frac{s \sin \beta}{s \cos \beta \sin^2 \theta_1 - \cos^2 \theta_1} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

(8) may be written

$$\tan^2 \theta_1 = \frac{1 + s \sin \beta \cot \phi}{s(\cos \beta - \sin \beta \cot \phi)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Eliminating θ_1 between (7) and (8) there is obtained finally, after solving for s,

$$s=\frac{\cos\beta(1+2\tan^2\phi)\pm 2\sec\phi\sqrt{\cos^2\beta\tan^2\phi-\sin^2\beta}}{4\sin^2\beta\sec^2\phi+\cos^2\beta}. \quad (10)$$

giving the relation between s, β, and ϕ, desired.

(10) may be written in a form more convenient for calculation thus—

$$s=\frac{\cos\beta\{2-\cos^2\phi\}\pm 2\sqrt{\cos^2\beta-\cos^2\phi}}{4-\cos^2\beta\{4-\cos^2\phi\}} \quad . \quad . \quad . \quad (11)$$

Again, when $\beta=0$ (11) may be written

$$s=\frac{1\pm\sin\phi}{1\mp\sin\phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

And it will also be observed that, when $\beta=\phi$, the quantity under the radical sign becomes zero, and when $\beta>\phi$, negative, rendering the whole expression imaginary from a physical point of view.

These equations give two values of s consistent with the conditions. Since, however, the reaction of the wall is a *passive* force, corresponding to the *active* pressure of the earth, it appears at once from Moseley's principle that the smaller value of s must be taken. Therefore, finally,

$$s=\frac{\cos\beta\{2-\cos^2\phi\}-2\sqrt{\cos^2\beta-\cos^2\phi}}{4-\cos^2\beta\{4-\cos^2\phi\}} \quad . \quad . \quad (13)$$

and

$$s=\frac{1-\sin\phi}{1+\sin\phi} \text{ when } \beta=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

Given therefore the angle of repose of the earth, and the corresponding friction angle for the earth on the wall, the direction and amount of the mutual action becomes determinate if we assume that failure cannot take place until β has attained its limiting value, which may be denoted by β_2. It may be at once remarked that, however closely β may approach to ϕ, it can never exceed that value; since, when $\beta=\phi$, the surface of separation, between the wall and the earth, becomes a plane of rupture. When, on the other hand, it is supposed with greater generality that at the time of fracture $\beta_2>\beta<\phi$, equilibrium must be destroyed without any relative motion between the wall and the earth in the immediate neighbourhood of the inner face having, in the first instance, taken place. In other words, the inclination denoted by β has not reached its limiting amount when failure occurs along one or both of the two possible planes of rupture which may be shown to exist.

The author ventures to advance the view that the condition of things may be rendered intelligible by a second application of Moseley's principle. For, consider the stability of the wall, and suppose the critical condition to have been attained, and that so before β had reached the value β_2. To the actual value of β looked upon for the moment as a limiting angle, there corresponds a certain roughness of the wall; and suppose that by some external agency (such as a change of temperature or the like) the degree of roughness of the actual wall to be, in effect, reduced to this value. It is impossible to conceive that by a process such as this the critical state can have been disturbed. On the other hand, there can be no difficulty in perceiving that, if it is a fact that friction between the earth and the wall adds to its stability, failure may be brought about by simply allowing the degree of roughness to fall ever so little below the critical value—the value, that is, which corresponds to the actual value of β when the wall is in the critical state. Hence β has a value such that the stability of the wall in the critical state is a maximum; or, in other words, the overturning moment of the earth pressure for any given wall is a minimum with respect to β.

In most cases which occur in practice, β may range through all values up to ϕ; for not only are the inner faces of retaining walls usually left rough, but they are frequently stepped in a manner which makes a cross section somewhat resemble a flight of steps.

The author has calculated some numerical values for s, corresponding to certain values of ϕ and β, which will be found in an annexed table. He has also tabulated the corresponding values of the ratio of breadth to depth of a wall whose density is equal to the density of the earth, and whose moment of stability is just equal to the overturning moment of the earth pressure. A polar curve showing the values of these ratios for $\phi = 40°$ corresponding to the successive values of β is also annexed.

Equation (14) resulting from (13) by putting $\beta = 0$, is identical with the corresponding formula of Rankine. The author has, however, farther verified the formula (13) and by consequence (6), for the case where $\phi = 60°$ and $\beta = 30°$. Inserting these values in (13),

$$s = \cdot 0853 \qquad \frac{1}{s} = 11 \cdot 723$$

To find the principal axes for this stress system, put $a = 0$ in (5), and let θ_a be the corresponding value of θ. This gives, on solution of the resulting quadratic,

$$\tan \theta_a = -\frac{f - \cos \beta}{2 \sin \beta} \pm \sqrt{\frac{(f - \cos \beta)^2}{4 \sin^2 \beta} + 1} \quad . \quad . \quad . \quad (15)$$

where f is written for $1/s$.

Hence,
$$\tan \theta_a = \cdot 046 \text{ or } -21 \cdot 760$$
$$\therefore \quad \theta_a = 2^\circ \, 38' \text{ or } -87^\circ \, 22'$$

Assigning to p_v for simplicity the value 10, and making use of (3) or (4),

$$r' = 10 \cdot 02 \qquad r'' = \cdot 718$$

where r' and r'' denote respectively the greatest and least principal stresses.

Now from Rankine's ellipse of stress it is seen at once that the obliquity has a maximum value on two sections symmetrically situated with respect to the axes of amount

$$\sin^{-1} \frac{r' - r''}{r' + r''},$$

which for the case in point becomes—

$$\sin^{-1} \frac{9 \cdot 302}{10 \cdot 738} = \sin^{-1} \cdot 866 = 60^\circ;$$

and this agrees with the original assumption. The relative positions of the two planes for which the obliquity is maximum, the principal axes, and the vertical and horizontal directions are shown in the diagram, upon which, also, the ellipse of stress for the system in question has been represented.

Suppose, for simplicity, that the cross section of the wall is rectangular. The oblique pressure p_o, inclined to the normal to the inner face of the wall at the angle β, is found, for any point, simply by multiplying the ratio s by the pressure due to the column of earth at that point. Let the earth have the uniform density ρ_e. Then the oblique pressure on the face at the depth h from the surface is

$$p_o = sh\rho_e.$$

The force on an element of area 1 foot long horizontally, and of depth dh is therefore

$$dP = sh\rho_e dh.$$

Hence the whole action of the earth on the wall per foot-run is represented by a force

$$P = \tfrac{1}{2}h_1^2 s\rho \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

where h_1 is the effective depth of the earth. P will act at the centre of pressure, and be inclined at the angle β to the horizontal. Let b be the breadth, h the height, and ρ_m the density of the wall. Then, equating the overturning moment to the moment of stability, and solving a quadratic

$$\frac{b}{h_1} = \frac{\rho_e}{\rho_m}\left\{ -\frac{s\sin\beta}{2} + \sqrt{\left(\frac{s\sin\beta}{2}\right)^2 + \frac{\rho_m}{\rho_e} s \frac{\cos\beta}{3}} \right\} . \quad . \quad . \quad (17)$$

A good example of the divergence of the results obtained by Rankine's formula from actual facts, even where the earth mass approximates to the hypothetical granular material of the mathematical investigation, is adduced by Mr Baker in his paper, "On the Actual Pressure of Earthwork," already referred to. The example in question is not only interesting in itself, but has the additional advantage of having been selected by Mr Flamant for an application of Professor Boussinesq's formulæ.

Mr Baker says—"When the wood paving was recently laid in Regent Street, the space being limited, the stacked wooden blocks in many cases had to do duty as retaining walls to hold up the broken stone ballast required for the concrete substructure. In one instance (Example I.) the author noted that a wall of pitch pine blocks, 4 feet high and 1 foot thick, sustained the vertical face of a bank of old macadam materials which had been broken up, screened, and tossed against this wall, until the bank had attained a height of 3 feet 9 inches, a width at the top of about 5 feet, and slopes on the farther sides, deviating little from 1·2 to 1" (*Proc. Inst. C.E.*, vol. lxv. p. 145). [1·2 to 1 corresponds to an angle of repose, 39° 48′].

Now, Rankine's hypothesis amounts to putting $\beta = 0$ in the notation of the present paper. Putting therefore in (17)

$\beta = 0.$

$h_1 = 3{\cdot}75.$

$\rho_e = 101$ lbs. per cub. foot. [Mr Baker.]

$\rho_m = 46 \times \dfrac{4}{3{\cdot}75} = 49$ lbs. per cub. foot. [Mr Baker.]

there is found

$$\frac{b}{h_1} = {\cdot}386 \quad \therefore\ b = 1{\cdot}448 \text{ ft.}$$

But actually the wall was only 1 foot thick; whereas, as has just appeared, according to Rankine's formula it would just have been on the point of overturning had it been 1·448 feet thick.

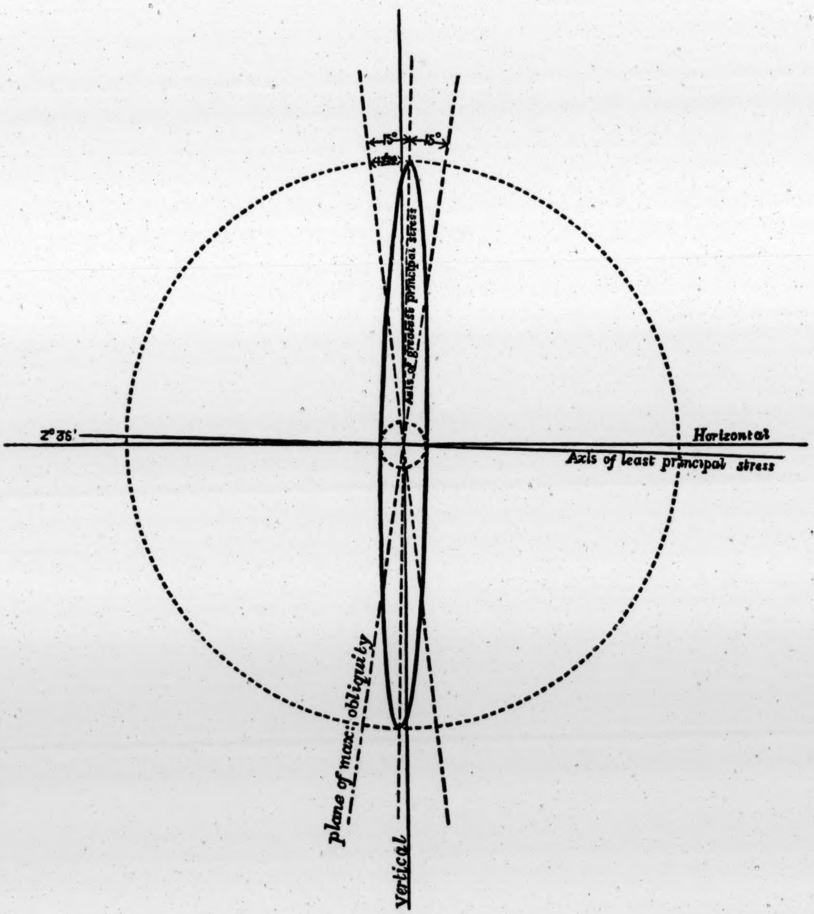

15°
15°
Axis of greatest principal stress
2° 35'
Horizontal
Axis of least principal stress
plane of max. obliquity
Vertical

Fig. 2.

According to the view adopted in this paper β will assume a value such that the overturning couple will be a minimum. Taking 39° 48′, as within the errors of observation 40°, an examination of

ϕ →	0°		5°		10°		15°		20°		25°		30°		35°		40°		45°	
β ↓	*s*	*b/h*	*s*	*b/h*	*s*	*b/h*	*s*	*b/h*	*s*	*b/h*	*s*	*b/h*	*s*	*b/h*	*s*	*b/h*	*s*	*b/h*	*s*	*b/h*
0°	1	·577	·840	·529	·704	·484	·589	·443	·490	·404	·406	·368	·333	·333	·271	·300	·217	·269	·172	·239
5°			·989	·532	·730	·461	·602	·429	·498	·385	·410	·351	·336	·320	·273	·289	·219	·260	·173	·232
10°					·956	·481	·651	·410	·522	·366	·425	·338	·345	·308	·279	·279	·223	·252	·175	·225
15°							·905	·435	·578	·363	·454	·328	·363	·298	·291	·271	·231	·243	·181	·219
20°									·841	·389	·512	·321	·394	·290	·309	·264	·243	·237	·189	·213
25°											·768	·346	·450	·286	·339	·256	·261	·231	·200	·207
30°													·693	·306	·392	·252	·289	·225	·218	·202
35°															·616	·270	·338	·222	·243	·197
40°																	·542	·236	·287	·196
45°																			·471	·206

the appended table shows that for $\rho_e = \rho_m$ this occurs when β is about 35°. No great error will be introduced by assuming that β has this value for the case in point. Taking the corresponding value of s from the table, and substituting in (17),

$$\frac{b}{h} = \cdot 2799 \text{ or } b = 1\cdot 05 \text{ ft.}$$

Professor Boussinesq's expressions gave b from 10 to $11\frac{1}{2}$ inches.

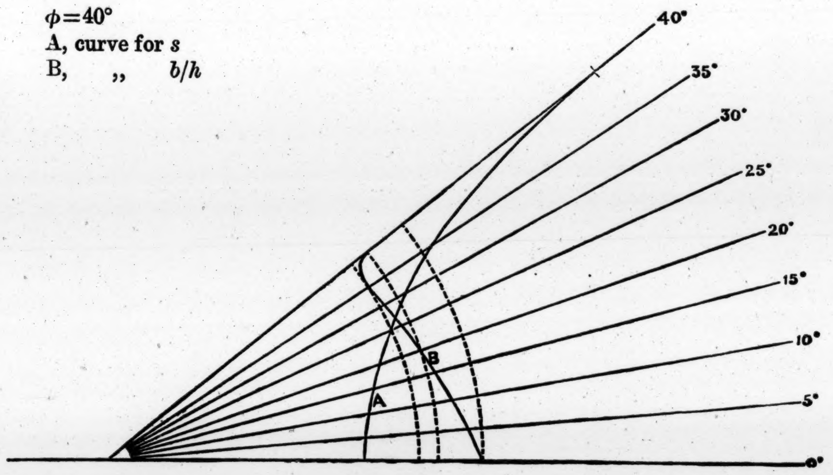


Fig. 3.

2. The Conducting Paths between the Cortex of the Cerebrum and the Lower Centres, in relation to their Function. By Professor D. J. Hamilton.

3. Researches on Micro-Organisms, including ideas of a New Method for their Destruction in Certain Cases of Contagious Disease. By Dr A. B. Griffiths, F.R.S. (Edin.), F.C.S. (Lond. & Paris), Principal, and Lecturer on Chemistry and Biology, School of Science, Lincoln; late Lecturer on Chemistry, Technical School, Manchester, &c.

The opinion of the most profound workers in the so-called "germ diseases" is, that these diseases are really due to the pathological effects of chemical substances (ptomaïnes) elaborated and secreted by certain micro-organisms. That is, a given contagious disease is rather the result of one or more compounds formed by the life-history of a micro-organism, than the mere presence of that micro-organism itself. By looking at certain cases in the above light, we can well understand why persons suffering with cholera die so rapidly. The alkaloid or ptomaïne (discovered by Pouchet in 1885) which the *Comma bacillus* secretes or forms is rapidly absorbed into the blood, long before the bacillus itself is capable of being absorbed by the mucous membrane of the intestine, and then into the blood. It is a well-known fact that most micro-organisms multiply with great rapidity in the media in which they live, and if particular organisms can be destroyed, the harmful effects of the products produced by their life-histories will *not* increase, and the disease will soon be at an end.

Of course, I am fully aware that it must *not* be supposed that because we find in the blood and tissues of man and animals (suffering from a contagious disease) certain micro-organisms, that these micro-organisms are necessarily the cause, or even indirectly the cause, of that disease. Not until we have obtained by pure cultivations, in an artificial sterilised medium, the organisms in a perfectly pure state, and then, by an injection into the blood of man or an animal of the purified organisms, the disease is reproduced, can we say that a particular disease is the result of the life-history of any particular micro-organism.

It is not my object here to describe the methods adopted by physiological chemists to obtain pure cultivations of any given micro-organism, although no interpretations can be given of any experiments unless the experimenter has worked with purified organisms obtained by artificial cultivation with all its precautions.

I wish to detail what appears to my mind the most reasonable method for the treatment of those contagious diseases whose "seat of war" is in the *blood* itself. I have already had the honour of presenting to the Royal Society of Edinburgh a paper "On the Action of Salicylic Acid on Ferments" (*Proc. Roy. Soc. Edin.*, No. 121, pp. 527–530). It was shown in that paper that an aqueous solution of salicylic acid (0·2 grm. of the acid in 1000 c.c. of water) was capable of destroying such micro-organisms as *Mycoderma aceti*, *Bacterium lactis*, and *Bacillus butyricus* (*B. amylobacter*). It was found, on a close microscopical examination with high powers, that the salicylic acid solution had acted upon the form of cellulose forming the external wall of these lowly organisms, perforating it, and ultimately destroying the life of the organisms.

During the last nine months I have turned my attention to the action of this salicylic acid solution upon other living micro-organisms.

First of all, I may state that when the above quantity of salicylic acid was added to 1000 c.c. of sterilised wort, and then pure cultivations of *Mycoderma aceti* were introduced into the wort contained in an ordinary two-necked Pasteur's flask (fig. 1), no change took place in it, not even after a month had elapsed, and the vessel kept all the time at the most suitable temperature (32° to 38° C.) for the life-history of the organism. After a month had passed, a small quantity of this wort was transferred (with all physiological caution) into a second Pasteur's flask containing sterilised wort *minus* the salicylic acid. No change was observed in the least, not after the elapse of a month. I experimented in a like manner with *Bacillus butyricus*, *Bacterium lactis*, and obtained similar results. From these experiments, and my previous microscopical studies, I draw the conclusion that the salicylic acid solution is an antiseptic agent destroying these low micro-organisms. In the case of *Bacillus butyricus*, I found that the said salicylic acid solution not only destroyed the organism, but its spores as well. My friend, Mr W. L. Gadd, F.C.S. (Principal Chemical Assistant to Dr W. Thomson, F.R.S.E., of the Royal Institution, Manchester), writes me that recently he has examined "*a sample of*

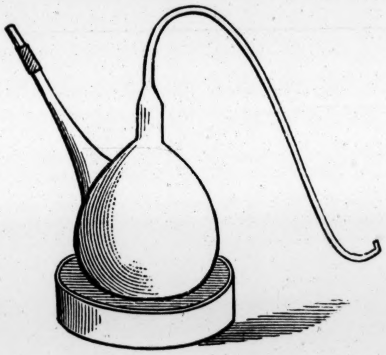


FIG. 1.—Pasteur's Two-Necked Flask.

damaged ginger beer which was quite thick and jelly-like." This injury was caused "*by a minute ferment which can easily be destroyed by salicylic acid*" (Gadd).

1. *Micrococcus prodigiosus.*

The spherical micro-organism (*Micrococcus prodigiosus*) which has been found upon cooked and raw meat kept in the dark, was found on transplanting into 1000 c.c. of sterilised beef-broth (neutral) to grow well in the dark, and formed a thick pellicle on the surface of the liquid. On the addition of 0·2 grm. of salicylic acid to the broth, the growth ceased; and on transferring a portion of this broth into another flask containing sterilised broth, no development took place, not after remaining in this fluid at a temperature of about 34 C. for two weeks. I may say in passing that I have found that the red pigment produced by this "chromogenic micrococcus" contains some compound of iron. On treating these organisms upon a slide under the microscope, with a weak solution of potassium sulphocyanate, a deeper red colour was produced, no doubt due to the formation of ferric sulphocyanate. Again, when a solution of potassium ferrocyanide was run between the slide and cover-slip containing *Micrococcus prodigiosus*, a blue coloration was obtained of "Prussian blue." It appears from the above that the colour produced by this micro-organism is some iron compound.

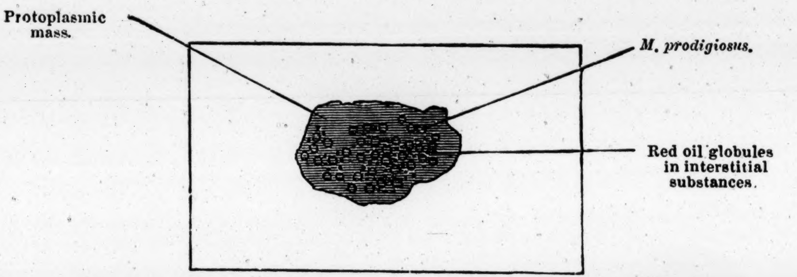


FIG. 2.—*M. prodigiosus* (zooglœa state) (much enlarged).

2. *Micrococcus aurantiacus and Bacterium aeruginosum.*

I have also found that the salicylic acid solution proved fatal to *Micrococcus aurantiacus* and *Bacterium aeruginosum*, the micro-organisms found upon badly-made bread, that has been kept in a damp place for three weeks. The first micro-organism produces an orange colour, and the second a green colour upon bread. These micro-organisms grow well in flour-paste at a temperature of about 34° C., forming a coloured skin upon the surface of the paste. If the paste is treated, first of all, with salicylic acid, and then *M. aurantiacus* and *B. aeruginosum* transplanted to the paste, they do *not* produce any growth, and ultimately die. The salicylic acid solution destroys the spores of these two micro-organisms, although their spores are said to withstand a temperature of 125° C.

3. *Micrococci found in Diarrhœa.*

The micrococci found largely in secretions from the bowels of persons suffering with diarrhœa were transplanted into sterilised beef-broth contained in a Pasteur flask (No. 1), and kept at 40° C. They multiplied rapidly during two days. Another flask (No. 2), containing sterilised beef-broth and salicylic acid (in the proportion of 0·2 grm. of the acid in 1000 c.c. of the broth), was taken and inoculated with these actively growing micrococci, but their growth was stopped. On inoculating another flask (No. 3) with some of the contents of flask (No. 2), and keeping it about 40° C. for three weeks, no further growth took place. This proves that the salicylic acid had killed the micrococci in question. I am fully aware that it has not been thoroughly ascertained whether this micrococcus is the cause of diarrhœa, yet, for my purpose, the above is to show that the said salicylic acid destroys this micrococcus.

4. *Leptothrix buccalis and Bacillus subtilis.*

These non-pathogenic forms found in the healthy human mouth are also destroyed by salicylic acid. When examined under the microscope, there was the appearance of the solution of salicylic acid perforating the cellulose wall of *Bacillus subtilis,* and so destroying the organism (fig. 3, B). At first we notice the cell-wall becomes thinner on the side that the salicylic acid solution is being run in between the slide and cover-slip, and is then perforated.

5. *Penicilium glaucum.*

Penicilium glaucum (the mould of preserved fruits, old leather, &c.). When mounted in a drop of water on the slide under the microscope, it was observed that on running in between the slide and cover-slip the solution of salicylic acid, the cellulose walls of hyphæ, conidia, conidiophores were all perforated, and ultimately dissolved by the acid.

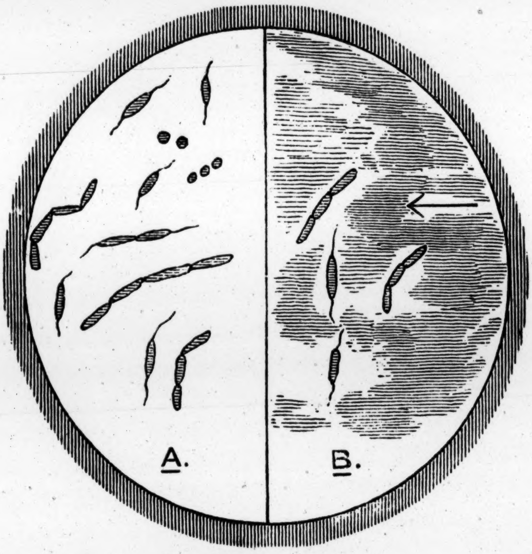


FIG. 3.—A, *Bacillus subtilis* (much enlarged). B, ←=Direction of flow of salicylic acid solution.

N.B.—In fig. 3 B the cell-walls should be thinner (than in woodcut) and perforated, especially on the observer's right.

6. *Vaccine Lymph.*

The micrococcus vacciniæ, which is the active principle of the vaccine virus (as shown by Pasteur, and not the fluid medium in which these micrococci live their life-histories), is also acted upon by salicylic acid, for the lymph so treated loses its power of inoculation.

7. *Micrococcus ureæ* (Von Tieghem).

Micrococcus ureæ, or the zymogenic ferment of urine which splits up urea into ammonium carbonate, is found in dumb-bells, chains, and in the zooglœa state. If urine is allowed to stand until it begins to smell of ammonia, and then a drop of this urine examined microscopically, the *Micrococcus ureæ* will be found to be present. If now fresh urine (sterilised) is treated with salicylic acid, and then inoculated with *Micrococcus ureæ* (from the putrid urine), no change at all occurs in the urine, not after the elapse of sixteen days. Therefore, I conclude that the said acid has acted upon this organism in a similar manner to those already mentioned.

8. *Protococcus vulgaris and Protococcus pluvialis* (fig. 4).

These two species of Protococcus are *not* acted upon by the solution of salicylic acid, and a much stronger solution of the acid has *no* effect upon them. It appears that there is a difference in the atomic structure of the cellulose forming the cell-walls of these two species of Protococcus and the various micro-organisms alluded to in this paper; that is, the cellulose of Protococcus is more like the cellulose of the higher forms of plant life, which is not acted upon by the acid in question.

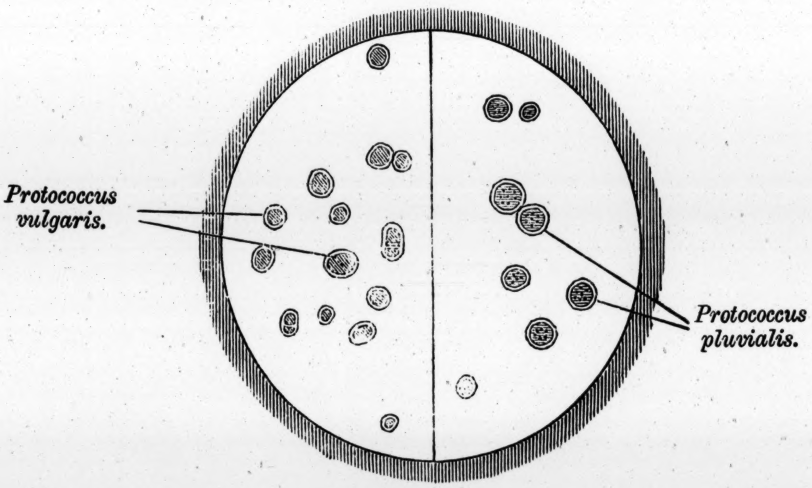


Fig. 4.

So far, we have seen that salicylic is an antiseptic agent, capable of attacking or acting upon their cellulose walls.

This acid, and acids generally, appear adverse to the life-histories of certain micro-organisms. It will be remembered that M. Bochefontaine swallowed secretions from choleraic patients containing *Comma bacilli*, made up in the form of pills, without any serious consequences. There is no doubt the *acid* properties of the gastric juice in the stomach had acted upon these micro-organisms in some way or other, and so prevented them living their life-histories in the intestine.

The practical outcome of this piece of research (although far from completed) may prove a remedy (placed upon a scientific basis) for certain contagious diseases whose organisms *reside in the blood.*

I may allude, in passing, that there is no science on such an unsatisfactory basis as medicine, and the present method of administering medicines (in such diseases as above) is first of all into the stomach by means of the mouth. The medicine has to pass through a good portion of the alimentary canal before it is absorbed into the blood. The medicine may become changed (before it is absorbed) by the various secretions pouring into the alimentary canal. Hence, for those *contagious* diseases whose seat of action is in the blood (the blood being their soil), we ought to apply our remedy directly to the blood itself by *injections.*

With this idea in view (and from analogy of the action of salicylic acid upon a large number of micro-organisms), it is most probable that the above salicylic acid solution will destroy *Micrococcus erysipelatosus*, *Micrococcus scarlatinæ*, *Bacillus malariæ*, *Bacillus tuberculosis*, &c. These micro-organisms have been proved to be the cause of the diseases in which they are found in great numbers in the blood. Swine fever, cattle plague, pleuro-pneumonia have also been proved to be the results of different species of micro-coccus. All these organisms reside in the blood of man and animals suffering with these diseases.

In the case of *Bacillus tuberculosis*, according to Dr E. Freund (*Nature*, vol. xxxiv. p. 581, Oct. 14, 1886), of Vienna, this micro-organism appears to form cellulose within the blood of tuberculous persons. This cellulose is a product of its (*B. tuberculosis*) life-history, and we have seen in numerous cases that the salicylic acid attacks micro-parasitic cellulose, if I can so use the expression. Therefore the hint may be given, that salicylic acid may prove a successful remedy in tuberculosis, when injected directly into the blood of diseased patients.

It may be asked—If the salicylic acid solution is injected into the blood, will it not destroy the red and white corpuscles? On April 28, 1886, I opened a vein in my left arm, and injected into the blood the solution of salicylic acid, and with the exception of a headache or so there were no abnormal results.

A microscopical examination of the blood (two hours after injection) revealed that the blood corpuscles were in a perfectly healthy state. The salicylic acid solution had *no* action upon the corpuscles of blood of a poor quality. Hence, I have reason to conclude that it may be, with a more extended study of the action of this solution of salicylic acid upon disease "germs" and their organisms, we have the most rational mode of treating those contagious diseases whose seat of energy is in the blood.

Note to above Paper.

The oven for sterilising tubes, cotton-wool, &c., I use in my experiments, was devised by my wife, and is seen in fig. 5. A good point of this oven is, that the shelf contains a series of holes

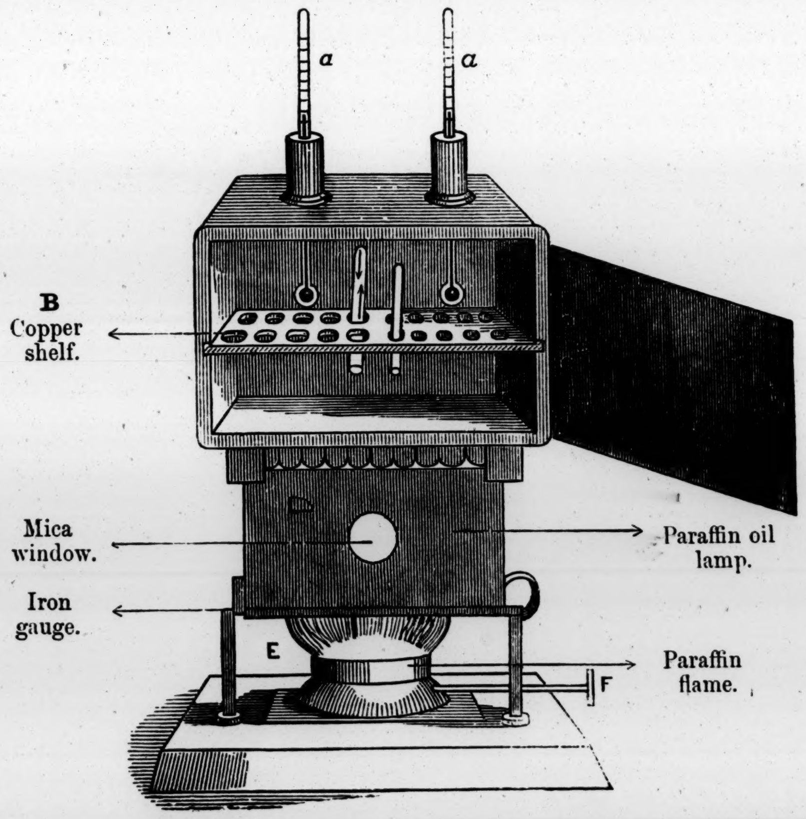


Fig. 5.—Mrs Griffiths' Form of Sterilising Oven. *a*, The ordinary chemical thermometers passing into oven; B, Copper shelf, with holes for tubes, cotton-wool, &c.; D, Iron support for oven over flame; E, Paraffin oil lamp; F, Screw to raise the wick, &c. The temperature of oven heated with paraffin oil can go as high as 155° C., and with gas, above 300° C.

of various sizes for test-tubes. By placing the test-tubes in inverted positions (as in fig.), the heated air rises in the tubes, and a current of air in each tube is formed, and thus destroys all organisms and their spores, for they are detached by these currents (of heated air rising), and exposed on all sides to the full heat of the oven.

Monday, 7th February 1887.

The HON. LORD M'LAREN, Vice-President, in the Chair.

1. On Cases of Instability in Open Structures. By E. Sang, LL.D.

(*Abstract.*)

In the course of some remarks on the design proposed for the Forth Bridge, the author of this paper had enunciated the remarkable theorem, that any symmetric structure built on a rectangular base, and depending on linear resistance alone, is necessarily unstable. The proof of it, given in the eleventh volume of the *Transactions of the Royal Scottish Society of Arts*, is derived from considerations affecting the special case; but this theorem is only one of an extensive class, and therefore the subject of instability among linear structures in general is here taken up.

In the case of regular or semi-regular arrangements, having the corners of an upper supported from the corners of an under polygon, it is shown that when the figures are of odd numbers the structures are stable, while those with even numbers are unstable; unless indeed the polygons be placed conformably, in which case the stability extends to both classes.

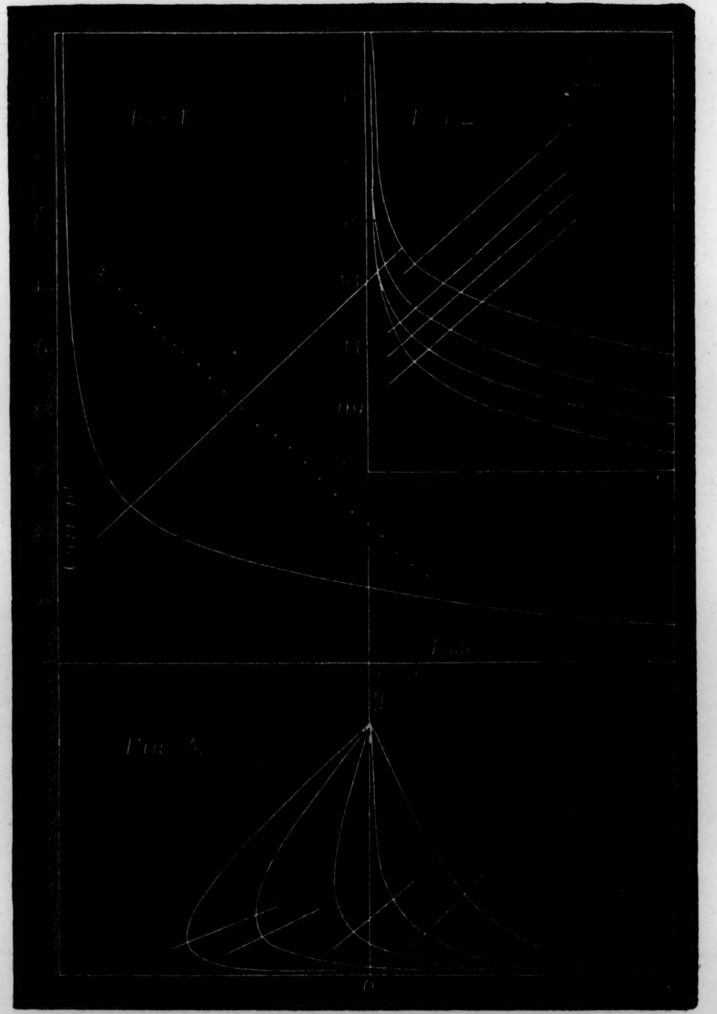
The paper ends with the following admonition:—These cases of instability in open structures have been elicited by means of the simplest considerations in geometry and statics; they lie indeed on the very surface of mechanical inquiry. They do not occur as isolated examples, but are arranged in extensive groups; and, being found in those classes of structures which may be called shapely, they stand out as warning beacons to those engaged in engineering pursuits.

2. On the Increase of Electrolytic Polarization with Time. By W. Peddie, Esq. Communicated by Professor Tait.

In a paper communicated to this Society last Session I described some preliminary experiments on this subject, which led to the approximate empirical formula connecting current-strength and time

$$i = a + b\epsilon^{-ct + d\epsilon^{-et}}$$

where a, b, c, d, and e are constants of positive sign. The electrodes used were of platinum, and had a surface area of about 5 sq. cm. Subsequently I have used platinum electrodes having an area of



rather more than 60 sq. cm. The curves I obtained evidently possess, to a considerable extent, symmetry about an axis. They roughly resemble hyperbolas, but have rather more curvature near the vertex. A few of them are represented in fig. 2. Time is measured horizontally, each scale-division representing 6¼ minutes. The numbers on the vertical scale, when multiplied by 0·267, give the current-strength in ampères. I used in each experiment a battery of 3 tray-Daniell cells, the electromotive force of which remained constant during the experiment. The battery was coupled in circuit with a Helmholtz tangent galvanometer and the electrolytic cell. The reduction factor of the galvanometer is 0·267. The needle of the galvanometer ceased vibrating in about half a minute after completing the circuit, at which time the first reading was taken. The following tables give the details of one experiment:—

Deflection in Degrees at intervals of One Quarter-Minute.

64·8, 64·4, 63·5, 62·7, 61·8, 60·9, 59·7, 58·8, 58·2, 58, 57·8, 57·4, 57, 56·7, 56·2, 55·7, 55·2, 54·6, 54, 53·6, 53·1, 52·7, 52·4, 51·9, 51·7, 51·4, 51·1, 50·9, 50·5, 50·3, 50, 49·8, 49·6, 49·4, 49·2, 48·9, 48·8, 48·6, 48·4, 48·3.

At intervals of One Half-Minute.

47·9, 47·6, 47·3, 47·1, 46·8, 46·6, 46·4, 46·2, 45·9, 45·8, 45·7, 45·6, 45·5, 45·3, 45·2, 45·1, 44·9, 44·8, 44·7, 44·5, 44·4, 44·3.

At intervals of One Minute.

44·1, 43·9, 43·7, 43·6, 43·4, 43·2, 43·1, 42·8, 42·7, 42·6, 42·5, 42·3, 42·2, 42·1.

At intervals of Three Minutes.

41·6, 41·1, 40·8, 40·3, 39·9.

At intervals of Five Minutes.

39·4, 38·7, 38·4, 37·8, 37·4, 36·9, 36·6, 36·4, 36, 35·7, 35·4, 35·1, 34·8, 34·5.

These results are shown graphically in the curve in fig. 1. In each experiment the total resistance in the circuit was slightly different. A special feature of the group of curves in fig. 2 is the close parallelism of the axes.

I have adopted, as a better approximation to the law connecting current-strength and time, the equation

$$i = a + b\epsilon^{ci-kt} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

The curve represented by this equation does not differ much from that represented by the equation formerly given. It possesses to a certain extent symmetry about an axis. In fig. 3 I have represented a group of curves having this equation, passing through a given point on the axis of i, and having a common asymptote. The curves which pass to the left of the axis of i cannot, of course, represent the physical phenomena, but they show the axial symmetry well. If another group is drawn, the axis of i being a tangent, and each member having a different horizontal asymptote, a close resemblance between it and the group in fig. 2 is evident.

If equation (1) holds, we have

$$\log (i - a) - ci = \log b - kt \quad . \quad . \quad . \quad . \quad (2).$$

Hence the quantity on the left-hand side of this equation plotted against time should give a straight line. The group of points in fig. 1 is obtained in this way from the curve in that figure. The near coincidence of all the points with a straight line shows that the curve is closely represented by an equation of the form (1).

I have also made an experiment with only one tray-Daniell cell in the circuit. There was, consequently, no decomposition of the electrolyte (an aqueous solution of sulphuric acid). In this case the constant a must be zero. The equation

$$i = 0{\cdot}4168\epsilon^{1{\cdot}092i - 0{\cdot}4015t}$$

represents the results of observation almost exactly, the difference being well within the limit of possible errors in drawing the curve free-hand through the observed points. For small values of t there is considerable discrepancy between the results of observation and calculation in both experiments, so that the formula (1) must not be applied when t is below a certain limit, which, for the results tabulated above, is about 5 minutes.

3. Astronomical Notes. By Ralph Copeland, Esq., Ph.D. Communicated by Lord M'Laren.

PRIVATE BUSINESS.

Dr J. B. Buist, Mr A. B. Brown, C.E., Mr Ferdinand Faithful Begg, Mr J. Arthur Thomson, Dr Andrew Thomson, Dundee; Mr William Caldwell Crawford, Mr J. G. Bartholomew, Dr William Hunter, Mr Thomas Goodall Nasmyth, Sir John Fowler, M. Inst. C.E., Mr W. H. Barlow, M. Inst. C.E., and Mr John Muter were balloted for, and declared duly elected Fellows of the Society.

Monday, 21st February 1887.

REV. PROFESSOR FLINT, D.D., Vice-President, in the Chair.

1. Further Determinations of the Effect of Pressure on the Maximum Density Point of Water. By Professor Tait.

2. On the Height of the Land of the Globe above Sea-Level. By Dr J. Murray.

3. Note on the Effects of Explosives. By Professor Tait.

(*Abstract.*)

Many of the victims of the dynamite explosion, a year or two ago, in the London Underground Railway, are said to have lost the drum of one ear only, that nearest to the source. This seems to point to a projectile, not an undulatory, motion of the air and of the gases produced by the explosion. So long, in fact, as the disturbance travels faster than sound, it must necessarily be of this character, and would be capable of producing such effects.

Another curious fact apparently connected with the above is the (considerable) finite diameter of a flash of forked lightning. Such a flash is always photographed as a line of finite breadth, even when the focal length is short and the focal adjustment perfect. This cannot be ascribed to irradiation. The air seems, in fact, to be driven outwards from the track of the discharge with such speed as to render the immediately surrounding air instantaneously self-luminous by compression.

Such considerations show at once how to explain the difference between the effects of dynamite and those of gunpowder. The latter is prepared expressly for the purpose of developing its energy gradually. Thus while the flash of gunpowder fired in the open is due mainly to combustion of scattered particles,—that produced by dynamite is mainly due to impulsive compression of the surrounding air, energy being conveyed to it much faster than it can escape in the form of sound.

4. Report on Fossil Fishes collected in Eskdale and Liddesdale. Part I. Ganoidei—Supplement. By Dr Traquair.

5. On the Equilibrium of a Gas under its own Gravitation only. By Sir W. Thomson.*

This problem, for the case of uniform temperature, was first, I believe, proposed by Tait in the following highly interesting question, set in the Ferguson Scholarship Examination (Glasgow, October 2, 1885):—"Assuming Boyle's Law for all pressures, form the equation for the equilibrium-density at any distance from the centre of a spherical attracting mass, placed in an infinite space filled originally with air; Find the special integral which depends on a power of the distance from the centre of the sphere alone."

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* *Note of February* 22, 1887.—Having yesterday sent a finally revised proof of this paper for press, I have to-day received a letter from Prof. Newcomb, calling my attention to a most important paper by Mr J. Homer Lane, "On the Theoretical Temperature of the Sun," published in the *American Journal of Science* for July 1870, p. 57, in which precisely the same problem as that of my article is very powerfully dealt with, mathematically and practically. It is impossible now, before going to press, for me to do more than refer to Mr Lane's paper; but I hope to profit by it very much in the continuation of my present work which I intended, and still intend, to make.—W. T.

The answer (in examinational style !) is :—Choose units properly; we have

$$\frac{d\rho}{dr} = -\frac{\rho\int_0^r \rho r^2 dr}{r^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

where ρ is the density at distance r from the centre. Assume

$$\rho = Ar^{\kappa} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

We find $A=2$, $\kappa = -2$; and therefore

$$\rho = \frac{2}{r^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

satisfies the equation in the required form.

Tait informs me that this question occurred to him while writing for *Nature* a review of Stokes' Lecture * on Inferences from the Spectrum Analysis of the Lights of Sun, Stars, Nebulæ, and Comets; and in the *Proceedings of the Edinburgh Mathematical Society* he has given some Transformations of the equation of Equilibrium. The same statical problem has recently been forced on myself by considerations which I could not avoid in connection with a lecture which I recently gave in the Royal Institution of London, on "The Probable Origin, the Total Amount, and the Possible Duration of the Sun's Heat."

Helmholtz's explanation, attributing the Sun's heat to condensation under mutual gravitation of all parts of the Sun's mass, becomes not a hypothesis but a statement of fact, when it is admitted that no considerable part of the heat emitted from the Sun is produced by present in-fall of meteoric matter from without. The present communication is an instalment towards the gaseous dynamics of the Sun, Stars, and Nebulæ.

To facilitate calculation of practical results, let a kilometre be the unit of length; and the terrestrial-surface heaviness of a cubic kilometre of water at unit density taken as the maximum density, under ordinary pressure, be the unit of force (or approximately, a thousand million tons heaviness at the earth's surface). If p be the pressure, ρ the density, and t the temperature from absolute zero, we have, by Boyle and Charles's laws,

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* Lecture III. of Second Course of "Burnet Lectures," Aberdeen, Dec. 1884; published, London, 1885 (Macmillan).

$$p = H\rho t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4);$$

where t denotes absolute (thermodynamic *) temperature, with 0° C. taken as unit; and H denotes what is commonly, in technical language, called "the height of the homogeneous atmosphere" at 0° C. For dry common air, according to Regnault's determination of density,

$$H = 7{\cdot}985 \text{ kilometres} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4').$$

Let β be the gravitational coefficient proper to the units chosen; so that $\beta mm'/D^2$ is the force between m, m' at distance D. The earth's mean density being 5·6, and radius 6370 kilometres, we have

$$\frac{4\pi}{3} . 6370 . 5{\cdot}6\beta = 1; \text{ and therefore } 4\pi\beta = 1/11890 \quad . \quad . \quad . \quad . \qquad (5).$$

Let now the p, ρ, t of (4) be the pressure, density, and temperature at distance r from the centre of a spherical shell containing gas in gross-dynamic † equilibrium. We have, by elementary hydrostatics

$$\frac{dp}{dr} = -\rho\left(M + \int_a^r dr 4\pi r^2 \rho\right)\beta/r^2 \quad . \quad . \quad . \quad . \qquad (6),$$

whence

$$\frac{d}{dr}\left(\frac{r^2}{\rho}\frac{dp}{dr}\right) = -4\pi\beta r^2 \rho \quad . \quad . \quad . \quad . \quad . \qquad (7),$$

where M denotes the whole quantity of matter within raidius a from the centre; which may be a nucleus and gas, or may be all gas.

If the gas is enclosed in a rigid spherical shell, impermeable to heat, and left to itself for a sufficiently long time, it settles into the condition of gross-thermal equilibrium, by "conduction of heat," till the temperature becomes uniform throughout. But if it were stirred

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* The notation of the text is related to temperature Centigrade on the thermodynamic principle (which is approximately temperature Centigrade by the air-thermometer), as follows :—

$$= \frac{1}{273}(\text{temperature Centigrade} + 273);$$

see my Collected Mathematical and Physical Papers, vol. i. arts. xxxix. and xlviii. part vi. §§ 99, 100; and article "Heat," §§ 35–38 and 47–67, *Encyc. Brit.*, and vol. iii. (soon to be published) of Collected Papers.

† Not in molecular equilibrium of course; and not in gross-thermal equilibrium, except in the case of t uniform throughout the gas.

artificially all through its volume, currents not considerably disturbing the static distribution of pressure and density will bring it approximately to what I have called convective equilibrium * of temperature, that is to say, the condition in which the temperature in any part P is the same as that which any other part of the gas would acquire if enclosed in an impermeable cylinder with piston, and dilated or expanded to the same density as P. The *natural stirring* produced in a great free fluid mass like the Sun's, by the cooling at the surface, must, I believe, maintain a somewhat close approximation to convective equilibrium throughout the whole mass. The known relations between temperature, pressure, and density for the ideal "perfect gas," when condensed or allowed to expand in a cylinder and piston of material impermeable to heat, are †

$$p = \mathrm{HT}\rho^k \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8),$$

$$t = \mathrm{T}\rho^{k-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9);$$

where k denotes the ratio of the thermal capacity of the gas, pressure constant, to its thermal capacity, volume constant, which is approximately equal to 1·41 or 1·40 (we shall take it 1·4) for all gases, and all temperatures, densities, and pressures; and T denotes the temperature corresponding to unit density in the particular gaseous mass under consideration.

Using (8) to eliminate p from (7) we find

$$\frac{d}{dr}\left[r^2\frac{d(\rho^{k-1})}{dr}\right] = -\frac{4\pi\beta(k-1)}{\mathrm{HT}k}r^2\rho \quad . \quad . \quad . \quad . \quad . \quad (10),$$

which, if we put

$$\rho^{k-1} = u \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11),$$

$$1/(k-1) = \kappa \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12),$$

and

$$r^{-1}\sqrt{\frac{\mathrm{HT}k}{4\pi\beta(k-1)}} = x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13),$$

takes the remarkably simple form

$$\frac{d^2u}{dx^2} = -\frac{u^\kappa}{x^4} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14).$$

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* See "On the Convective Equilibrium of Temperature in the Atmosphere," *Manchester Phil. Soc.*, vol. ii., 3rd series, 1861; and vol. iii. of Collected Papers.

† See my Collected Mathematical and Physical Papers, vol. i. art. xlviii. note 3.

Let $f(x)$ be a particular solution of this equation; so that

and therefore
$$\left.\begin{aligned} f''(x) &= -[f(x)]^{\kappa}x^{-4} \\ f'(mx) &= -[f(mx)]^{\kappa}m^{-4}x^{-4} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (15).$$

From this we derive a general solution with one disposable constant, by assuming

$$u = Cf(mx) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16);$$

which, substituted in (14), yields in virtue of (15),

$$m^2 = C^{-\kappa+1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17);$$

so that we have, as a general solution,

$$u = Cf[xC^{-\frac{1}{2}(\kappa-)}] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18).$$

Now the class of solutions of (14) which will interest us most is that for which the density and temperature are finite and continuous from the centre outwards to a certain distance, finite as w shall see presently, at which both vanish. In this class of cases u increases from 0 to some infinite value, as x increases from some finite value to ∞. Hence if $u = f(x)$ belongs to this class, $u = Cf(mx)$ also belongs to it; and (18) is the general solution for the class. We have therefore, immediately, the following conclusions :—

(1) The diameters of different globular* gaseous stars of the same kind of gas are inversely as the $\frac{1}{2}(\kappa-1)$th powers (or $\frac{3}{4}$ powers) of their central temperatures, at the times when, in the process of gradual cooling, their temperatures at places of the same densities are equal (or "T" the same for the different masses). Thus, for example, one sixteenth central temperature corresponds to eight-fold diameter; one eighty-first central temperature corresponds to twenty-seven fold diameter.

(2) Under the same conditions as (1) (that is, H and T the same for the different masses), the central densities are as the κth powers

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* This adjective excludes stars or nebulæ *rotating steadily* with so great angular velocities as to be much flattened, or to be annular; also nebulæ revolving circularly with different angular velocities at different distances from the centre, as may be approximately the case with spiral nebulæ. It would approximately enough include the sun, with his small angular velocity of once round in 25 days, were the fluid not too dense through a large part of the interior to approximately obey gaseous law. It no doubt applies very accurately to earlier times of the sun's history, when he was much less dense than he is now.

(or $\frac{5}{2}$ powers) of the central temperatures; and therefore inversely as the $\frac{2\kappa}{\kappa-1}$, or $\frac{2}{2-k}$, or $\frac{10}{3}$, powers of the diameters.

(3) Under still the same conditions as (1) and (2), the quantities of matter in the two masses are inversely as the $\left(\frac{2}{2-k}-3\right)$th powers (inversely as the cube roots) of their diameters.

(4) The diameters of different globular gaseous stars, of the same kind of gas, and of the same central densities, are as the square roots of their central temperatures.

(5) The diameters of different globular gaseous stars of different kinds of gas, but of the same central densities and temperatures, are inversely as the square roots of the specific densities of the gases.

(6) A single curve $[y=f(r^{-1})]$ with scale of ordinate (r) and scale of abscissa (y) properly assigned according to (18), (17), and (11) shows for a globe of any kind of gas in molecular equilibrium, of given mass and given diameter, the absolute temperature at any distance from the centre. Another curve $\{[y=f(r^{-1})]^\kappa\}$, with scales correspondingly assigned, shows the distribution of density from surface to centre.

It is easy to find, with any desired degree of accuracy, the particular solution of (13), for which

$$u=\mathrm{A},\ \text{and}\ \frac{du}{dx}=\mathrm{A}',\ \text{where}\ x=a \quad . \quad . \quad . \quad . \quad (19),$$

a denoting any chosen value of x, and A and A′ any two arbitrary numerics, by successive applications of the formula

$$u_{i+1}=\mathrm{A}-\int_a^x dx\left(\mathrm{A}'-\int_a^x dx\frac{u_i^\kappa}{x^4}\right) \quad . \quad . \quad . \quad (20);$$

the quadratures being performed with labour moderately proportional to the accuracy required, by tracing curves on "section"-paper (paper ruled with small squares) and counting the squares and parts of squares in their areas. To begin, u_0 may be taken arbitrarily; but it may conveniently be taken from a hasty graphic construction by drawing, step by step, successive arcs * of a curve with radii of

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* This method of graphically integrating a differential equation of the second order, which first occurred to me many years ago as suitable for finding the shapes of particular cases of the capillary surface of revolution, was successfully carried out for me by Prof. John Perry, when a student in my

curvature calculated from (13) with the value of du/dx found from the step-by-step process. If this preliminary construction is done with care, by aid of good drawing-instruments, u_1 calculated from u_0 by quadratures will be found to agree so closely with u_0, that u_0 itself will be seen to be a good solution. If any difference is found between the two, u_1 is the better: u_2 is a closer approximation than u_1; and so on, with no limit to the accuracy attainable.

Mr Magnus Maclean, my official assistant in the University of Glasgow, has made a successful beginning of working out this process for the case $u=16$ where $x=\infty$; and has already obtained a somewhat approximate solution, of which the produce useful for our problem is expressed in the following table:—

Numerical Solution of $\frac{d^2u}{dx^2}+x\;{}^{4}u^{2\cdot5}=0.$

Distance from centre $=r=1/x$.	Reciprocal of distance from centre $=x=1/r$.	Temperature $-u$.	Density $=u^{2\cdot5}$.	Mass within distance r from the centre $=du/dx$ $=\int_x^\infty dxu^{2\cdot5}x_{-\cdot4}$
0	8	16·00	1024	·00
·100	10	14·46	795·2	·28
·111	9	14·14	751·6	·38
·125	8	13·71	695·8	·52
·143	7	13·10	621·2	·731
·167	6	12·20	520·0	1·056
·200	5	10·92	394·1	1·566
·250	4	9·00	243·0	2·336
·333	3	6·15	93·81	3·436
·500	2	2·25	7·595	4·366
·667	1·5	0	0	4·49

The deduction from these numbers, of results expressing in terms of convenient units the temperature and density at any point of a given mass of a known kind of gas, occupying a sphere of given radius, must be reserved for a subsequent communication.

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laboratory in 1874, in a series of skilfully executed drawings representing a large variety of cases of the capillary surface of revolution, which have been regularly shown in my Lectures to the Natural Philosophy Class of the University of Glasgow. These curves were recently published in the *Proc. Roy. Instit.* (Lecture of Jan. 29, 1886), and *Nature*, July 22 and 29, and Aug. 19, 1886; also to appear in a volume of Lectures now in the press, to be published in the *Nature* series.

One interesting result which I can give at present, derived from the first and last numbers of the several columns of the preceding table, is, that the central density of a globular gaseous star is $22\frac{1}{2}$ times its average density.

Monday, 7th March 1887.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Equilibrium of a Gas under its own Gravitation only. Part III. By Sir W. Thomson.

2. History of the Theory of Determinants. Part I. Determinants in General: Hindenburg (1784) to Reiss (1829). By Dr Muir.

[This Paper is printed at the end of the *Proceedings* of the Session.]

3. Note on Solar Radiation. By Mr John Aitken.

In the many theories that have been advanced to explain the comparative constancy of solar radiation in long past ages as evidenced by geological history, it has been generally assumed that the temperature of the sun has not varied much, and to account for its not falling in temperature a number of theories have been advanced, all suggesting different sources from which it may have received the energy which it radiates as heat. Since the chemical theory was shown to be insufficient to account for the vast amount of heat radiated, other theories, such as the meteoric theory and the conservation of energy theory, have been advanced.

In all these theories it is generally assumed that in order that the radiation from the sun may be constant its temperature must also be constant, that, in fact, radiation from the sun is in proportion to its temperature; and that if the temperature of the sun falls, its radiation effect at the earth's surface will fall in direct proportion to its loss of heat. Now there are certain physical facts which have induced me to think that this is not necessarily so. Nay, it seems even possible that the amount of heat radiated by the sun might go on increasing while its temperature was decreasing.

The facts which seem to point to this conclusion are—

First, We know that different forms of matter vary greatly in their power of radiating heat. For instance, a non-luminous gas flame radiates far less heat than a luminous one, though actually at a higher temperature, and a red-hot surface of platinum radiates far less heat than an oxidised surface of iron of the same area and temperature.

Second, We know that elementary bodies generally radiate far less heat than compound ones. It has been shown that the radiating powers of substances go on increasing with the increased complexity of their constitution.

Third, We know that at high temperatures compound bodies are decomposed or broken up into simpler forms. Or, to put it in another way, and as a solar inhabitant would put it, bodies which have an affinity for each other do not combine unless their temperature is below a certain fixed point for the substances.

Combining these statements we see that in the sun, owing to its high temperature, matter must be in much simpler forms than it is on the earth, and all recent investigation points markedly in this direction. It seems therefore in the highest degree probable that the average radiating power of the matter in the sun is much less than that of the matter of the earth. Again, the hotter the sun the simpler its constitution will be, and the weaker its radiating power. From this we see that there is no necessary proportion between the temperature of the sun and the amount of heat it radiates, as change of temperature is accompanied by change of constitution, and increase of radiating power.

These considerations lead us on, and suggest that the store of energy from which the sun has drawn in long past ages may possibly be its own internal supply; that the sun in its earlier ages was at a much higher temperature than it is at present, but, owing to its simpler constitution, it radiated only about as much heat as it does now, and that as its temperature fell its matter became more compound, and its radiating power increased, which enables it now to keep up about the same radiation effect, though its temperature may have fallen greatly.

It is evident that much of this is purely theoretical and formed on rather a weak basis; we have so few measurements to go by. Though it is a law that compound bodies radiate more than simple ones, yet we do not know enough of the constitution of the sun to say how much the increased radiating power, due to increased complexity, would compensate for the fall of temperature. I have said it might actually more than counteract the fall of temperature and cause an increased radiation effect. It may, however, only balance it, or it may even only reduce the rate of decrease of radiation, and make it only a little less than proportional to the fall in temperature.

The whole of these remarks are almost pure speculation. The principal cause for their being written is to point out that the radiating power of the sun may have varied in quantity and quality from age to age; that its amount may not be directly proportional to its absolute temperature; and further, that it is extremely doubtful whether we can apply to solar matter the radiation measurements which have been obtained of earth matter, so that any estimates we may make of the temperature of the sun from measurements of solar radiation must be received with considerable hesitation.

Added 9*th July* 1887.

Sir William Thomson has shown that the sun has within itself an enormous store of energy due to its high temperature. This energy is altogether apart from what we on the earth are accustomed to consider as the energy stored up in a hot body. He has shown that the shrinkage or falling in of the great mass of the sun due to cooling is capable of developing a very great amount of energy. His calculations show, that if the shrinkage was so great that the surface of the sun was to descend at the rate of 35 metres per year, or 70 kilometres per 2000 years, there would be developed about the same amount of energy that is radiated by the sun according to the measurements taken by Pouillet.

Forbes has shown Pouillet's measurements to be much too small, and that the amount radiated by the sun is 1·6 greater than

Pouillet supposed. Professor Langley, by more perfect methods, has shown that Forbes's figure is also too small, and there are evident reasons for supposing that even Langley's measurements are too low. The sun will therefore require to shrink a good deal more than 35 metres per year to develop from gravitational sources alone the energy radiated by it.

Apart, however, from the energy developed by shrinkage, there will evidently be energy developed within the sun in another way while it is cooling. The falling temperature will be accompanied by combustion, though not in the manner supposed in the old combustion theory of solar energy. There will, however, evidently be a development of energy due to the combination or falling together of the molecules which will ensue on the decrease of temperature. Here we have a field for the chemist to come in and do for the chemical part of the subject what Sir W. Thomson has done for the gravitational. It must, however, be confessed that his task is a far more difficult one, at least at present, as we know very little about the condition of matter in the sun, and almost nothing about the amount of energy developed when the simpler forms of matter combine.

Now, though the sun may receive an enormous amount of energy from those two sources, yet it is evident they can never keep the temperature of the sun constant, because, before energy can be developed in either of those ways, the temperature of the sun must fall, and the energy developed will be in proportion to the amount of the fall.

4. On Laplace's Nebular Theory, considered in relation to Thermodynamics. By Sir W. Thomson.

5. On a Class of Alternating Functions. By Dr Muir.

6. Note on Hoar-Frost. By Mr John Aitken.

Hoar-frost is generally described as frozen dew, and is supposed to be deposited in the same manner and under the same conditions as dew; the only difference being in the temperature at which it is deposited. Though in a general way this may be so, yet there are certain differences in the conditions, and the manner in which the vapour is condensed at the different temperatures, which seem worth referring to.

If we examine a surface, such as a sheet of glass, exposed horizontally near the ground on a dewy night, we shall generally find that the windward edges are dry. This indicates that the air itself is not cooled to the dew-point, though the surfaces of bodies exposed to radiation are, and the air has to travel some distance over the cold surface before its temperature is reduced to the dew-point. If, however, we examine this same surface when the temperature is low enough to cause the deposited moisture to form hoar-frost, we shall frequently find a marked difference. The sheet of glass is generally not only covered with the deposited vapour up to the windward edges, but the deposit is heaviest along these edges, the ice crystals growing furthest out in that direction. This peculiarity in the deposition of the hoar-frost may also be observed on almost all objects—such as branches of trees, iron fences, &c. The heaviest deposit will often be found on the windward side and not on the top, where we might expect to find it, owing to the stronger radiation from that surface.

The question then is, What is the cause of this difference? Why should no vapour be deposited along the windward edges of surfaces when the temperature is above 32°, while the heaviest deposit is formed on these edges when the temperature is below the freezing-point? The dryness of that part of the dewed surface where the air first touches it is caused by the air not being saturated, and requiring to travel some distance over the cold surface before it is cooled below its dew-point. When, however, hoar-frost is forming, the air seems generally to act as if it were supersaturated: the crystals growing most towards the wind seems to indicate that the air does not require to be cooled before it deposits its moisture. But is it possible for the air to be supersaturated? Under ordinary conditions we know this is impossible. Owing to the vast amount of dust in the air there is always plenty of *free-surface* present to prevent this happening so long as the temperature is above the freezing-point. When, however, the temperature falls below this point, we have a much more complicated condition of matters.

A considerable time ago it was suggested by Professor James Thomson and by Kirchhoff that the vapour pressure of ice might be less than that of water at the same temperature; Professor Ramsay and Dr Young have shown that this is the case,* and they have experimentally measured the comparative temperatures of ice and water under the same vapour pressure. This they have done to a temperature of 9 degrees below the freezing-point; lower they could not go, as the water always froze when its temperature was reduced to that point. They found that the ice and the water had the same temperature at 32°, and under a pressure of 4·6 mm. But when the pressure was still further reduced, the water became colder than the ice; and when the pressure was about 3·20 mm. the water was at a temperature of about 23°, while the ice was about 24°. The water was thus about a degree colder than the ice.

It is evident, therefore, that if by any means the ice had been cooled to the same temperature as the water, its vapour-pressure would have been less than that of the water. So that if we have a water-surface and an ice one at the same temperature and near each other, vapour will tend to pass from the water to the ice, because, the vapour pressure of the water being higher than that of ice, the air which is saturated to a water surface is supersaturated to an ice one.

Something like this seems to take place when hoar-frost is forming. When the air is cooled, condensation takes place on the dust nuclei, resulting in a foggy condensation. This moisture condensed in the air seems always to keep the liquid form; at least we do not see during frosty weather any indications of the particles being frozen. In the fogs formed low down in our atmosphere there are no optical or other phenomena such as we might expect to find if they were frozen. That the temperature of the air is far below the freezing-point is no evidence that the fog particles will be solid, as it is well known that water, even when in contact with solid surfaces, and with what seem favourable nuclei for forming freezing centres, may yet remain liquid at a temperature far below the freezing-point. Thin films and small drops seem difficult to freeze; I have frequently seen my night-radiation thermometer cooled many degrees below the freezing-point, and yet the film condensed on its surface was in a liquid state. It seems, therefore, quite in keeping with our knowledge that these fog particles in frosty weather may be liquid.

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* *Phil. Trans. Roy. Soc.*, part ii., 1884.

Such being the case, we have water particles floating in the atmosphere during frosty and foggy weather, and the pressure of the vapour in the air will correspond to that of a liquid surface; it will therefore be greater than that of an ice one at the same temperature. Under these conditions the air will rapidly unburden itself of part of its vapour when it comes into contact with an ice surface. This seems to be the reason why hoar-frost grows in the direction from which the air is moving, because the air, being supersaturated, unburdens itself on the first ice surface with which it comes into contact, and does not, as when dew is forming, require to be brought into a condition to cause it to give up its vapour.

In the foregoing I have taken extreme conditions under which dew and hoar-frost are formed, as they are better suited to illustrate the point. There are, however, many intermediate conditions in which both dew and hoar-frost appear to be deposited in nearly the same way. On some nights the sheet of glass is dewed all over and up to all the edges, and there are some nights on which no hoar-frost is deposited on the windward edges of the plate, the air having to pass some distance over the cold surface before its temperature is low enough for it to deposit its moisture. The conditions under which the plate is dewed to its edges are when there is no wind and the air nearly saturated; and the conditions under which no hoar-frost is deposited along the windward edges are when there is some wind, a clear sky, and the air not saturated.

So far as my memory and recorded observations go, we never have a heavy deposit of hoar-frost when the sky is clear, or in those conditions in which we have our heaviest deposits of dew. On all those occasions on which trees and every exposed surface become clothed in crystal garments, and all nature in a single night becomes changed to a wondrously pure and fairy-like scene, the transformation seems always to be accomplished in a thick and foggy atmosphere, which requires the morning's sun to dissolve the veil and disclose its beauties. The thick and foggy state seems to be the general condition of our atmosphere during the growth of these heavy deposits of hoar-frost, and it is a necessary one, if the explanation we have given is correct.

It will be observed that these thick and foggy nights, when heavy deposits of hoar-frost are formed, are the very nights on which little or no dew would be deposited, because the radiation would be checked by the fog. These, however, are the very nights most favourable for the deposit of hoar-frost, because the air has a large amount of vapour in it,—is in fact saturated. And further, while dew requires that the surface on which it is deposited be cooled by radiation, this is not so necessary, and indeed may be absent, in the formation of hoar-frost; because the fog particles radiate and cool the air to the saturated temperature of vapour at a water surface, and the passing air discharges part of its vapour on all ice crystals or other nuclei with which it comes in contact; the passing air at the same time absorbs the heat of crystallisation, while the heat of condensation is balanced by the heat absorbed by the evaporation from the water particles.

But, further, it will be observed that not only are those nights on which hoar-frost is most abundant not similar to the nights on which heavy dews are formed, but they are generally nights on which there would be no dew at all if the temperature was above the freezing-point; these hoar-frosty nights do not therefore correspond to dewy nights, when the temperature is higher, but rather to those nights when every object is wet and dripping, not with dew, but wet with deposited fog particles.

7. On the Quotient of a Simple Alternant by the Difference-Product of the Variables. By Dr Muir.

8. Investigations on the Influence of certain Rays of the Solar Spectrum on Root-Absorption and on the Growth of Plants. By Dr A. B. Griffiths, F.R.S.E., F.C.S. (Lond. & Paris), and Mrs A. B. Griffiths.

This paper details an investigation undertaken to see the influence of certain rays of white light on root-absorption and assimilation in the vegetable kingdom. One of us has been for some years investigating a problem as to the use of ferrous sulphate as a plant food (see Dr Griffiths' memoirs in Journal of Chemical Society

[*Trans.*], 1883, 1884, 1885, 1886, 1887). In the experiments to be detailed here, we have used ferrous sulphate as an indicator of root-absorption.

In seven small flower-pots, each filled with the same kind of soil, and treated with a known weight of ferrous sulphate, were sown some mustard seeds. When the little plants had made their appearance above the soil, they were exposed for some hours each day to the coloured lights of the spectrum.

No. 1 was exposed to the red part of the spectrum; No. 2 to the orange, and so on; after so many hours' exposure they were removed to a dark place. This operation was performed for several weeks, each pot being exposed to its own part of the spectrum daily until the plants had grown to a considerable size. The plants in each pot were then reduced to ashes, and submitted to analysis, the following percentage of ferric oxide being found :—

MUSTARD PLANTS.

				Percentage of Fe_2O_3 in Ash.
Pot No. 1	exposed to	red part of spectrum,	gave . .	0·92
,, 2	,,	orange	,, . .	1·43
,, 3	,,	yellow	,, . .	2·51
,, 4	,,	green	,, . .	1·90
,, 5	,,	blue	,, . .	0·71
,, 6	,,	indigo	,, . .	0·20
,, 7	,,	violet	,, . .	0·15

The same number of seeds were placed in each pot, and each pot received the same quantity of iron sulphate during the investigation. The plants, as they grew, were watered from time to time with a weak solution of ferrous sulphate, always of the same strength (see figure).

We have also tried the same experiments upon bean seeds (working exactly under similar conditions as those detailed above), with the following results :—

BEAN PLANTS.

				Percentage of Fe_2O_3 in Ash.
Pot No. 1	exposed to	red part of spectrum,	gave . .	1·40
,, 2	,,	orange	,, . .	2·75
,, 3	,,	yellow	,, . .	4·52
,, 4	,,	green	,, . .	3·34
,, 5	,,	blue	,, . .	1·12
,, 6	,,	indigo	,, . .	0·84
,, 7	,,	violet	,, . .	0·53

The soil used in the fourteen experiments was a calcareous soil of the following composition:—

Lime,	53·00
Organic matter,	1·50
Oxides of iron and alumina,	1·62
Magnesia,	0·32
Potash and soda,	0·01
Phosphoric acid,	0·04
Silica,	0·26
Chlorine,	0·01
Sulphuric acid,	None.
Carbonic acid,	43·24
	100·00

GRAPHIC REPRESENTATION OF THE AMOUNT OF FERRIC OXIDE IN THE ASHES OF PLANTS AFTER GROWING IN AN IRON MANURE, AND EXPOSED TO VARIOUS RAYS OF THE SPECTRUM, ILLUSTRATING ROOT-ABSORPTION.

These experiments therefore show that the most active rays for root-absorption coincide with those of assimilation. From the researches of Drs Draper and Pfeffer, and those of Cloez and Gratiolet, it is evident that the most favourable rays of white light are those lying between the yellow and green. In their experiments the greatest amount of oxygen was evolved in this part of the spectrum, therefore the largest amount of carbon is retained by the process of assimilation. From our experiments we obtain the largest percentage of ferric oxide in the ashes of those plants exposed to the yellow or yellow-green part of the spectrum, or between Fraunhofer's lines D and E; therefore conclude that the most energetic rays of white light for root-absorption coincide with those for assimilation.

In the presence of light, or the different rays of white light, the two processes go on side by side.

ALBUMINOID FORMATION.

The mode of formation of albuminoids in the vegetable kingdom is said to be unknown. We know, according to Lieberkühn, that the albumens have an empirical formula of $C_{72}H_{112}N_{18}SO_{22}$. As this molecule contains *sulphur*, the increase of protoplasmic (albuminoids) matter in the living plants, as far as the sulphur is concerned, would only have come from the sulphur of the ferrous sulphate, as there were no other sulphates present in the soil. Moreover, we find the largest percentage of albuminoids in the plants when they have been grown in the yellow part of white light, as the following table shows:—

PERCENTAGE OF ALBUMINOIDS.

	Mustard Plants.	Bean Plants.
Pot No. 1 exposed to red part of spectrum, gave . .	23·3 %	2·4 %
,, 2 ,, orange ,, . .	25·5	4·2
,, 3 ,, yellow ,, . .	27·6	6·5
,, 4 ,, green ,, . .	26·2	5·3
,, 5 ,, blue ,, . .	21·1	3·6
,, 6 ,, indigo ,, . .	18·4	2·2
,, 7 ,, violet ,, . .	13·0	1·8

Also, the percentage of sulphur (estimated directly) was the largest in each case when grown in the yellow part of the spectrum.

Dr W. J. Russell, F.R.S. (of St Bartholomew's Hospital, London), found that in those plants grown by the aid of iron sulphate the relative amount of chlorophyll (in equal areas, and also in equal weights of the leaves) is increased over those not grown with the iron manure. The following table gives—

DR RUSSELL'S ESTIMATION OF RELATIVE AMOUNT OF CHLOROPHYLL IN THE LEAVES OF DR GRIFFITHS' CROPS OF 1884.

	Chlorophyll in equal areas of the leaves.	Chlorophyll in equal weights of the leaves.
(1) Beans grown *with* iron sulphate,	100	100
(2) ,, *without* ,,	79	76
(1) Turnips grown *with* ,,	59	61
(2) ,, *without* ,,	40	39

(For further details, see Journal Chemical Society, *Trans.*, 1885, page 54). This proves that a soluble iron salt nourishes the chlorophyll granules; and recently Prof. J. v. Sachs, in describing the symptoms of vegetable chlorosis, recommends the salts of iron as a remedy (Biedermann's *Centralblatt für Agrikultur Chemie*, vol xv. part 9). It has been shown by one of us that crystals of iron sulphate have been found near to the chlorophyll granules when sections of plants are examined under the highest powers of the microscope (Journal Chemical Society, *Trans.*, 1883, page 195).

Hence it appears that albuminoids, or, in other words, the protoplasm of the living cell, is formed by the combined action of assimilation and root-absorption in the vicinity of the chlorophyll granules. The sulphur required to complete the albumen molecule comes from the decomposition of sulphates, and the nitrogen from the nitrates or ammonia salts (added to the soil or derived from organic matter) taken into the plant by root-absorption.

PRIVATE BUSINESS.

Mr Arthur Silva White, Mr W. Peddie, Mr H. M. Cadell, Mr G. B. Wieland, and Mr A. H. Sexton were balloted for, and declared duly elected Fellows of the Society.

Monday, 21*st March* 1887.

The HON. LORD M'LAREN, Vice-President, in the Chair.

The following Communications were read:—

1. Variations in the Value of the Monetary Standard.
By Professor Nicholson.

2. On Ice and Brines. By J. Y. Buchanan.

(*Abstract.*)

The composition of the ice produced in saline solutions, and more particularly in sea-water, has frequently been the object of investigation and of dispute. It might be thought that to a question of whether ice so formed does or does not contain salt, experiment would at once give a decisive answer. Yet, relying on experiment alone, competent authorities have given contradictory answers. All agree that ice, whether formed artificially in the laboratory by freezing sea-water, or found in nature as one of the varieties of sea-water ice, retains, in one form or another, and with great tenacity, some of the salt existing in solution in the water. The question at issue is whether this salt is to be attributed to the solid matter of the ice or to the liquor mechanically adhering to it, from which it is impossible to free it. Most bodies, and especially those which take a crystalline form, are easily purified and freed from all suspected foreign matter, with a view to analysis, by the simple operation of washing and drying. It is impossible to wash the crystals, formed by freezing a saline solution, with distilled water, because they melt at a temperature below that at which distilled water freezes. The effect of the addition of a small quantity of distilled water to a quantity of saline ice is at first the anomalous one, that what was a wet sludge is transformed into a dry crystalline powder. It is, of course, impossible to dry the ice by heat, and to do so by more intense freezing would be begging the question. The experimental difficulties therefore account for some of the divergence of opinion on the subject. The mixed character of the substances examined has also much to do with it. As a rule, it may be said that those investigators who have confined their observations to the laboratory have concluded that the ice forming when saline solutions of moderate concentration, including sea-water, are frozen, is pure ice, and the salt from which it is impossible to free it entirely belongs to the mother-liquor, while those who have collected and examined sea-water ice in high latitudes have come to the opposite conclusion.

During the Antarctic cruise of the "Challenger" I made a number of observations on the sea-water ice found in those regions, and, relying principally on the fact that the melting temperature of the ice was markedly lower than that of fresh-water ice, and that it was impossible by any of the ordinary means familiar to chemists for freeing crystals from adhering mother-liquor to materially reduce its salinity, I came to the conclusion that the ice forming in freezing sea-water is not a mixture of pure ice and brine, but that it contains the salt found in it in the solid state either as a crystalline hydrate or as the anhydrous salt, but most probably as a hydrate. In dealing with the subject, Dr Otto Pettersson (*Water and Ice*, p. 302) quotes my observations, and also rejects the view that "sea-ice is in itself wholly destitute of salts, and only mechanically incloses a certain quantity of unfrozen and concentrated sea-water." He founds his belief on the fact that numerous analyses of specimens of sea-water ice have shown that the constitution of the saline contents of different specimens of ice differs for each specimen, and is always different from that of the saline contents of sea-water. Were the salinity due to inclosed unfrozen and concentrated sea-water, we "ought to find by chemical analysis exactly the same proportion between Cl, MgO, CaO, SO_3, &c., in the ice and in the brine as in the sea-water itself." He quotes numerous analyses of specimens of sea-water ice from the Baltic and from the Arctic Seas to show that this is not the case. Calling the percentage of chlorine in each case 100, he found in various sea-waters the percentage of SO_3 to vary from 11·49 to 11·89. In specimens of sea-water ice it varied from 12·8 to 76·6, and in brines separating from the ice and remaining liquid at − 30° C. it varied from 1·14 to 1·16.

This argument appears conclusive. In order to explain all the phenomena observed in connection with sea-water ice he cites Guthrie's investigations, which went to show that, in freezing saline solutions, under a certain concentration, pure ice is formed at a temperature which falls from 0° C., when the amount of salt dissolved is infinitely small, to a certain definite temperature when the solution contains a certain definite percentage of salt. Further abstraction of heat then produces solidification of the solution as a whole, in the form of a crystalline hydrate, of constant freezing- and melting-point. To such hydrates, Guthrie gave the name of cryohydrates. Pettersson quotes the following as being particularly applicable to the case of sea-water :—

The cryohydrate of	Contains per cent. of water.	Solidifies at °C.
NaCl	76·39	−22
KCl	80·00	−11·4
$CaCl_2$	72·00	−37·0
$MgSO_4$	78·14	− 5·0
Na_2SO_4	95·45	− 0·7

And he refers more particularly to the cryohydrate of Na_2SO_4 forming and melting at $-0°·7$.

Now the bearing of Guthrie's experiments is to show that, while at sufficiently low temperatures, and with suitable concentration, the water will solidify along with one or other of the salts in solution, until this low temperature and high concentration are attained, pure ice must be the result of freezing.

The abnormal phenomena attending the formation and the melting of ice in saline solutions and sea-water find a natural explanation in an observation which I have frequently quoted, and which Dr Pettersson mentions in a footnote at p. 318, namely, that "a thermometer immersed in a mixture of snow and sea-water which is constantly stirred indicates $-1°·8$ C." If this is true, it is clear that my melting-point observations proved nothing. On repeating the experiment I found it confirmed, and took the opportunity this winter of investigating the matter more closely. The paper now communicated to the Royal Society of Edinburgh contains the first portion of the results. It deals with the subject under two heads, namely, (*a*) the temperature at which sea-water and some other saline solutions freeze, and the chemical constitution of the solid and the liquid into which they are split by freezing; and (*b*) the temperature at which pure ice melts in sea-water and in a number of saline solutions of different strengths.

(*a*) The freezing experiments were limited to sea-water and solutions of NaCl comparable with sea-water.

Chloride of Sodium.—Four solutions were used, and they were intended to contain 3, 2·5, 2, and 1·5 per cent. NaCl respectively. Forty grammes of this solution, in a suitable beaker, were immersed in a freezing mixture of such composition as to give a temperature from 2° to 2°·5 C. below the freezing temperature expected. The temperature at which ice began to form (if necessary after adding a minute splinter of ice) was noted, and the freezing was allowed to continue with constant stirring till the temperature had fallen 0°·2 C. A specimen of the mother-liquor was removed, and the chlorine in it determined; the chlorine in the original solution had been determined before. The beaker was then removed from the freezing bath and allowed to melt. The temperature in all cases rose during melting exactly as it had fallen during freezing. In the following table are given the means of the temperature at which ice began to form in the original solution, and that of the liquid when the sample of brine was taken, and the means of the chlorine found in the original solution and in the brine sample :—

Mean freezing temperature, .	−1°·875 C.	−1°·63	−1°·30	−0°·975]
Mean per cent. Cl., . .	1·87	1·60	1·30	0·98

It will be seen that, in the dilute solutions experimented with, the percentage of chlorine expresses, in terms of the Centigrade scale, the lowering of the freezing-point of the solution.

Sea-Water.—Similar experiments were made with sea-water of different degrees of concentration. In sea-water from the Firth of Clyde containing 1·84 per cent. of chlorine, ice forms at −1°·9 C. The following results are from means of close-agreeing results :—

	°	°	°	°
Freezing temperature, .	−2·0	−1·5	−1·0	−0·5
Per cent. chlorine, .	1·94	1·445	0·963	0·475
Difference, . . .	0·06	0·055	0·037	0·025

Sea-water resembles a chloride of sodium solution containing the same percentage of chlorine, and the resemblance is closer the greater the dilution. When the beaker was removed from the freezing-bath, the temperature rose during melting as it had fallen during freezing. In these experiments, which had for their object the determination of the temperature at which the crystals melted, as well as that at which they began to form in the water, it was impossible to remove a sample for analysis large enough to enable the sulphuric acid to be determined in it.

For this purpose a series of observations were made, using quantities of 300 grammes of sea-water. Freezing was continued usually until the temperature had fallen 0°·3 C. below that at which crystals began to form. The mother-liquor was then separated from the crystals by means of a large pipette with fine orifice, before removing the beaker from the freezing bath. The magma of crystals was then brought rapidly on a filter and drained by means of the jet pump. The ice, thus drained, was then melted, and the three fractions were analysed. In the following table (I.) the results of four experiments are given. In the one column (W) will be found the weight of the original water taken and of the fractions into which it was split on freezing ; in the other (R) will be found the ratio of SO_3 to Cl found by analysis, the chlorine being set down as 100; thus, in I. the percentage of chlorine found in the crystals, melting at the lowest temperature, was 1·497, and that of the SO_3, 0·174; the ratio (R) is therefore 11·62.

TABLE I.—*Freezing Sea-Water—Analyses of Fractions.*

No. of Experiment,	I.		II.		III.		IV.	
Nature of Water,	Forth 100 °/₀.		Mother-liquor.		Clyde 100 °/₀.		Clyde 50 °/₀.	
	W.	R.	W.	R.	W.	R.	W.	R.
Original water,	300	11·83	90	11·67	300	11·58	300	11·21
Mother-liquor, .	170·6	11·67	...	11·83	102	11·57	78	11·67
Drainings, .			...	...	94	11·56	109	...
Crystals, . .	106	11·62	23	11·22	97	11·67	106	11·4
" . .	22·5	11·11	...	...	...	...	...	...

It will be seen that the ratios (R) found for mother-liquor, drainings, and ice agree with one another quite as closely as those found in samples of pure sea-water from different localities. It is to be remembered that in these experiments the water was frozen *gently*—that is, the rate of abstraction of heat was low, the temperature of the freezing bath being regulated so as to be about 2° C. below the freezing temperature of the solution. Much of the error and uncertainty about the freezing of saline solutions arises from the violence of the methods employed. Judging then by the constancy of the relation of the percentage of Cl to SO_3, we see that in sea-water, frozen at moderate temperatures, the composition of the saline contents of the original water, the mother-liquor, and the ice is identical; and we are justified in concluding that it is probable that the saltness of the ice is due to unfrozen and concentrated sea-water adhering to it. Ice forming in even very weak saline solutions closely resembles snow (which is ice forming in air), and has the same remarkable power of retaining mechanically several times its weight of water or brine.

If we assume that the ice formed in freezing sea-water is pure ice, and that the saline ingredients are retained by the portion remaining liquid, we can calculate the amount of ice which has been formed if we know the salinity of the original water and that of the residual brine. In the case of sea-water the salinity varies directly with the percentage of chlorine. The weight of the brine remaining after any freezing operation is found by multiplying the weight of the original water used by the ratio of the chlorine percentage found in the original water to that found in the brine. The difference between the weight so found and that of the original water is the weight of the ice formed. In Table II. the results of this calculation are given for Experiment III. on pure sea-water from the Clyde, and for Experiment IV. on the same water diluted with an equal weight of distilled water.

TABLE II.—*Calculation of Ice formed, on the basis of the Salinity of the Original Water and of the Residual Brine.*

No. of Experiment, . . .		III.	IV.
Weight of original water (grammes), . .	W	300	300
Per cent. Cl in ditto, ...	c	1·836	0·923
Per cent. Cl in mother-liquor, ...	K	2·212	1·153
Weight of mother-liquor, . . $W\frac{c}{K}=$	L	249·0	293·3
Weight of ice, ... $W-L=$	I	51·0	60·7

Sea-water, like other saline solutions, is easily cooled several degrees below its freezing-point before crystals begin to form. While cooling down to and below what was known to be its freezing-point, simultaneous observations of the temperature of the sea-water and the freezing-bath were made from half minute to half minute. From these observations, the rate of abstraction of heat for different differences of temperature of sea-water and bath was found. At a given moment a minute splinter of ice (weighing much less than a drop of water) was introduced. Crystals immediately began to form, and the temperature rose in from ten to fifteen seconds to the freezing-point. During the freezing the temperatures of bath and sea-water were observed at regular intervals. The heat removed is thus made up of that eliminated during the few seconds when freezing began and the temperature rose to the freezing-point, which is found by multiplying the rise of temperature by the weight of liquid, and that removed during the subsequent cooling, which is found from the duration of the operation and the rate of loss of heat, deduced from observations made during the cooling. The specific heat of the solution is taken as that of the water which it contains, namely 0·965 for III., and 0·9825 for IV. The mean freezing temperature was – 2°·05 for III. and – 1°·05 for IV. The latent heat of water freezing at 0° is 79·25. The specific heat of ice being 0·5, the latent heat of water freezing at – 2°·05 is 78·22, and that of water freezing at – 1°·05 is 78·73. In Experiment III. the total heat extracted during the freezing was $4230 \times 0{\cdot}965 = 4082$ heat units (gramme-degrees), and dividing this by 78·22 we find 52·2 grammes as the weight of pure ice formed at – 2°·05 C., equivalent to this abstraction of heat. In Experiment IV. the heat abstracted was found to be $5193 \times 0{\cdot}9825 = 5102$. Dividing this by 78·73 we find 64·8 grammes as the equivalent weight of ice formed.

We have calculated the weight of ice which would be found, first on the basis of the salinity of the solution; and second, on the basis of the observed thermal exchange, assuming in both cases that, in the act of freezing pure ice is formed. Thus—

TABLE III.

No. of Experiment, . . .	III.	IV.
	grammes.	grammes.
Calculated from thermal exchange, . .	52·2	64·8
,, ,, salinity, . .	51·0	60·7
Difference, ...	1·2	4·1

The agreement between the two quantities of ice formed as calculated by the different methods is as close as could be expected, and renders probable the truth of the common assumption that the solid body formed is pure ice.

It has, moreover, been proved by Guthrie, Rüdorff, and others that, in solutions of the salts occurring in sea-water, ice does separate out at first, and continues to separate out until the concentration has become many times greater than that of sea-water. Assuming that in sea-water all the chlorine is united to sodium, 85 per cent. of the water would have to be removed as ice before a cryohydrate would form, and if it contained nothing but sulphate of soda in the proportion corresponding to the sulphuric acid found in it, over 90 per cent. of the water would have to go as ice, before the cryohydrate would be formed.

In my experiments about 15 per cent. of the weight of the water was frozen out as ice, causing a lowering of freezing-point by 0°·3 C.

In nature it is probable that the ice forming at the actual freezing surface does so at an almost uniform temperature, the local concentration produced by the formation of a crystal of ice being immediately eliminated by the mass of water below. In the interstices of the crystals there will be retained a weight of slightly concentrated sea-water at least as great as that of the ice crystals. These retain the brine in a meshwork of cells, and, as the thickness of the ice covering increases, and the freezing surface becomes more remote, the ice and the brine become more and more exposed to the atmospheric rigours of the Arctic winter. The brine will continue to deposit ice until its concentration is such that, for example, the cryohydrate of NaCl is ready to separate out. It probably will separate out until it comes in conflict with, for instance, the chloride of calcium or the chloride of magnesium, which will retain some of the water, without solidifying, even at the lowest temperatures. At the winter quarters of the "Vega" brine was observed oozing out of sea-water ice and liquid at a temperature of – 30° C. It was very rich in calcium and especially magnesium chlorides. In fact, *it is probably quite impossible by any cold occurring in nature to solidify sea-water.*

b. Melting of pure ice in sea-water and other saline solutions.—A large number of experiments were made with solutions of concentration comparable with that of sea-water, and in one or two cases the experiments were extended to low temperatures and strong solutions. As a rule, from 50 to 100 grammes of solution, cooled to 0° C., were mixed with an equal weight of pounded ice, also at 0° C. The thermometer used for all these determinations was one of Geissler's normal ones, divided into tenths of a degree Centigrade; and its zero-point was verified almost daily. Along with the thermometer, a pipette of suitable capacity was immersed in the beaker, and used with the thermometer for keeping the mass well mixed. Its upper aperture was closed with a small cork, which was removed from time to time to permit of some of the brine being sucked up and allowed to run back again. The inside of the pipette was thus kept constantly moistened with the slowly altering solution in the beaker. The temperature was read after very thorough mixing, and the sample thereupon immediately removed and preserved for analysis.

As a rule, samples were taken for analysis at intervals of 0°·4 C. The results for three classes of salt in dilute solutions are arranged in Tables IV., V., and VI.

TABLE IV.—*Giving the percentage of Chlorine in Solutions of various Chlorides in which Ice melts at given Temperatures.*

Temperature of melting Ice.	Chloride in Solution.						
	HCl	NaCl	KCl	(Sea-Water.)	$MgCl_2$	$CaCl_2$	$BaCl_2$
	Per cent. Chlorine in Solution.						
° C.							
−3.5	3·06	3·30	...	...	4·12	...	...
−3·0	2·68	3·02	3·00	...	3·62	3·70	...
−2·5	2·28	2·53	2·50	...	3·12	3·20	...
−2·0	1·85	2·02	2·00	...	2·62	2·70	2·72
−1·5	...	1·50	1·50	1·500	1·19	2·15	2·10
−1·0	...	1·02	1·02	1·034	1·51	1·50	1·47
−0·5	...	0·50	0·52	0·588	0·87	...	...

TABLE V.—*Giving percentage of Potassium in Solutions of various Potassium Salts in which Ice melts at given Temperatures.*

Temperature of melting Ice.	Salt in Solution.			
	KCl	KI	$K\frac{Cl+I}{2}$	KOH.
	Per cent. K in Solution.			
° C.				
−3·0	3·29	3·02	3·15	...
−2·5	2·79	2·59	2·68	2·60
−2·0	2·28	2·13	2·17	2·08
−1·5	1·74	1·63	1·66	1·57
−1·0	1·18	1·13	1·12	...
−0·5	0·59	0·60	0·57	...

TABLE VI.—*Giving percentage of Hydrogen in Solutions of various Hydrogen Salts in which Ice melts at given Temperatures.*

Temperature of melting Ice.	Salt in Solution.			
	H_2SO_4	HCl	HNO_3	HKO
	Per cent. H in Solution.			
° C.				
−3·0	0·144	0·076	0·077	...
−2·5	0·119	0·065	0·065	0·066
−2·0	0·097	0·052	0·052	0·053
−1·5	0·073	...	0·042	0·041
−1·0	0·048	...	0·032	...

On considering them, it was at once evident that the lowering of the melting-point of ice followed the concentration of the solution, but the law deviated in all cases from that of strict proportionality to the amount of salt dissolved, in some cases to a greater extent than in others. In comparing the effects of different salts in solution on the melting-point of ice, no simple connection could be traced between their absolute weights and the effects produced; but on comparing chemically equivalent weights, a very close connection was discovered. This will be evident from the inspection of the tables. In each the first column contains the temperatures at which pure ice melts; and in the parallel columns the percentages of chlorine, potassium, or hydrogen in the solutions of the salts indicated at the head of each column, when ice melts in them at the temperature indicated. The figures thus give numbers proportional in each table to the chemically equivalent weights of the different salts. They show at first that, whereas the presence of equal absolute weights in solution produces very different effects, the presence of chemically equivalent weights produces very similar effects. On closer inspection, it is seen that the effects are almost identical where the elements to which the common constituent is united belongs to the same group of the periodic series, and differ sharply where these elements belong to different groups. In the case of the chlorides of sodium and potassium the number expressing the percentage * of chlorine in the solution expresses equally the depression of the melting-point of ice in terms of the Centigrade scale. The same depression of melting temperature is produced by 10 per cent. less of chlorine united to hydrogen, and by 30 to 35 per cent. more of chlorine when united to magnesium, calcium, or barium.

The results obtained with sea-water are also given, for comparison. It will be seen that it behaves very approximately as a solution of chloride of sodium containing the same amount of chlorine.

It is perhaps not very astonishing that unit weight of potassium in saline solution should produce the same effect in lowering the melting-point of ice, whether it is united to Cl or I; but it shows clearly how independent this action is of the general character of the body in solution when we find the effect produced by unit

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* All percentages are *by weight.*

weight of hydrogen identical, whether it is united to such opposite radicles as Cl or OK. Table VI. shows further the effect of valence. While a given weight of hydrogen produces the same effect in solution, whether it be united to the very different but both univalent radicles Cl and OK, its effect is reduced by one-half when united to the bivalent SO_4. That valence is not the only factor is shown by comparing the effects of hydrogen and potassium when united to the common element, chlorine. Hydrochloric acid in solution produces a markedly more powerful lowering effect on the melting-point of ice than the equivalent amount of chloride of potassium. Of all the substances that I have experimented on, hydrochloric acid is the most energetic in reducing the melting-point of ice, and with ordinary strong acid and pounded ice there is no difficulty in producing temperatures as low as the freezing-point of mercury. In the case of hydrochloric acid, sulphuric acid, chloride of sodium, and chloride of calcium, I have carried my experiments to low temperatures and great concentration. But before passing to them it is well to consider the more dilute solutions with regard to their density.

That the mere density of the solution in which the ice is melting has no direct connection with the lowering of its melting-point is shown by the following table, in which the specific gravities (at 15° C.) are given of the solutions of different salts which gave the same depressions of melting-point :—

Temperature of melting.	Specific Gravity of Solutions of				
	$NaCl$	KCl	$MgCl_2$	$CaCl_2$	$BaCl_2$
° C.					
−2·86	1·03370	1·03850	1·03893	1·04756	...
−1·8	1·02174	1·02535	1·02715	1·03262	1·06633

There are many similarities in the effects produced by greatly increasing the pressure upon pure water and by dissolving salts in it. First, there is an absolute diminution in the volume of the solution as compared with the sum of the volumes of its components; second, in virtue of this compression by molecular forces it has become less compressible by mechanical means; third, the temperature of maximum density and the freezing temperature are lowered; and fourth, the former of these two temperatures is lowered more rapidly than the latter. All these effects are produced *in kind* by increasing the pressure on pure water. Whether, or in how far, they agree in degree must be decided by future experiments.

TABLE VII.

Temperature of melting Ice.	Salt dissolved.		
	HCl	NaCl	$CaCl_2$
	Per cent. Cl in Solution.		
° C.			
−35	15·26		
−30	13·98	...	15·97
−25	12·60	...	14·47
−20	11·00	...	12·65
−15	9·17	11·10	11·29
−10	7·02	8·40	8·93
− 5	4·15	4·72	5·65

Experiments with Concentrated Solutions.—Several series of experiments have been made with hydrochloric acid, chloride of sodium, and chloride of calcium, and also with sulphuric acid. Table VII. gives the results, in the same form as preceding tables, for the chlorides.

It will be seen that, in proportion as the solution becomes more concentrated, further additions of salt produce a greater effect in lowering the melting-point of ice, and at a temperature of − 15° C. equivalent weights of NaCl and $CaCl_2$ produce identical results. In Table VIII. the results for hydrochloric acid and sulphuric acid are given in terms of the percentage of hydrogen in the solution.

TABLE VIII.

Temperature of melting Ice.	Acid dissolved.	
	HCl	H_2SO_4
	Per cent. H in Solution.	
−25° C.	0·355	0·538
−20	0·310	0·487
−15	0·258	0·418
−10	0·198	0·332
− 5	0·117	0·205

The temperatures given in these tables are all in terms of the same thermometer, which has not been verified for this part of its scale by comparison with a standard or with the air thermometer.

We have thus seen that, owing to its peculiar physical properties, it is impossible to prepare the crystalline solid which separates from sea-water and analogous saline solutions in a condition to enable the question, whether the salt does or does not form part of the solid matter of the crystals, to be solved directly by chemical analysis.

So far as chemical analysis is applicable, it is in favour of the salt belonging exclusively to the adhering brine. When sea-water is carefully frozen artificially, the ratio between the chlorine and the sulphuric acid is the same for the solid contents of the original water, the crystals, and the mother-liquor. It is exceedingly unlikely, if part of the salt went into the crystals, leaving the remainder in the brine, that there would be no selective separation of its constituents.

It has been shown that snow or pure lake ice, which, when melting by itself or immersed in pure water at atmospheric pressure, melts at the constant temperature called 0° C. or 32° Fahr., changes its melting temperature when immersed in a saline solution. The altered melting temperature, however, is the same for solutions of the same composition (no doubt with some allowance for pressure) and different for solutions of different composition.

The temperature at which pure ice melts in a solution is identical with that at which ice separates from the same solution on being sufficiently cooled.

When sea-water is frozen to the extent of 15 per cent. of its mass, and the crystals so formed are allowed to melt in the liquid in which they have been produced, they melt exactly as they have been formed. If snow or pure ice be immersed in the brine formed by partially freezing sea-water, it melts at the same temperature as the ice which had been formed by freezing the sea-water, so long as the chemical composition remains the same in each case.

The heat removed in freezing sea-water to the extent of 15 per cent. of its mass accounted for the production of the same amount of ice as was given by calculation on the basis of the chlorine found in the mother-liquor.

When some saline solutions are cooled for a sufficient length of time at a sufficiently low temperature, there arrives a certain concentration at a certain temperature, when further removal of heat causes solidification of the brine as a whole (cryohydrate).

The concentration necessary for the solidification of even the cryohydrate of highest melting temperature is such that in the *primary* freezing of the water of the sea no such body can be formed. It would follow from this consideration alone that the first ice formed on the sea in Arctic regions consists of pure ice, and it is also certain that it would retain a large quantity of the residual sea-water in its interstices. During the winter this inclosed liquor would solidify in the interstices of the crystals to ice and cryohydrates, in so far as the temperature and the nature of the salts in solution would permit. From my experiments with chloride of calcium, and the existence of brines observed to remain liquid at − 30° C. at the winter quarters of the "Vega," it is unlikely that sea-water, as a whole, can ever be completely solidified in nature. The presence of unfreezable or difficultly freezable brine in freshly-formed sea-water ice, explains its eminently plastic character even at very low temperatures.

The fact that cryohydrates of different salts solidify and melt at different temperatures, sufficiently explains the various composition of different specimens of *old* sea ice.

The apparent expansion, near the melting-point, of ice formed by freezing water which contains any salt at all is perfectly explained on the hypothesis that in the act of freezing the water rigidly excludes all saline matter from participation in its solidification.

The residual and unfreezable brine which remains in considerable quantity liquid when sea-water is frozen, must also remain in greater or less quantity when fresh water is frozen. All natural waters, including rain-water, contain some foreign and usually saline ingredients. If we take chloride of sodium as the type of such ingredients, and suppose a water to contain a quantity of this salt equivalent to one part by weight of chlorine in a million parts of water, then we should have a solution containing 0·0001 per cent. of chlorine, and it would begin to freeze and to deposit pure ice at a temperature of − 0°·0001 C.; and it would continue to do so until, say, 999,000 parts of water had been deposited as ice. There would then remain 1000 parts of residual water, which would retain the salt, and would contain, therefore, 0·1 per cent. of chlorine, and would not freeze until the temperature had fallen to − 0°·1 C. This water would then deposit ice at temperatures becoming progressively lower, until, when 900 more parts of ice had been deposited, we should have 100 parts residual water, or brine as it might now be called, containing 1 per cent. of chlorine, and remaining liquid at temperatures above − 1°·0 C. When 90 more parts of ice had been deposited, we should have 10 parts of concentrated brine containing 10 per cent. chlorine and remaining liquid as low as − 13° C. In the case imagined, we assume the saline contents to consist of NaCl only, and with further concentration the cryohydrate would no doubt separate out and the mass become really solid. On reversing the operations, that is, warming the ice just formed, we should, when the temperature has risen to about − 13° C., have 999,990 parts ice and 10 brine containing 10 per cent. chlorine. Now, owing to the remarkable fact that pure ice, in contact with a saline solution, melts at a temperature which depends on the nature and the amount of the salt in the solution, and is identical with the temperature at which ice separates from a solution of the same composition on cooling, the brine liquefies more and more ice at progressively rising temperatures, until, as before, when the temperature of the mass has risen to − 0°·1 C., it consists of 999,000 parts of ice and 1000 parts of liquid water, containing 1 part of chlorine. The remainder of the ice will melt at a temperature gradually rising from − 0°·1 to 0° C.

The consideration of this example furnishes an easy explanation of the anomalous behaviour of ice formed from anything but the very purest distilled water, in the neighbourhood of its melting-point. This subject has been studied with great care and thoroughness by Pettersson. The apparent expansion of all but the very purest ice, when cooled below 0° C., is ascribed by him in part to solid saline contents of the ice which exercise a disturbing and unexplained influence on its physical properties. Viewed in the light of the fact that the presence of even the smallest quantity of saline matter in solution prevents the formation of ice at 0° C., and promotes its liquefaction at temperatures below 0° C., we see that this apparent expansion of the ice on cooling is probably due to the fact that we are dealing, not with homogeneous solid ice, but with a mixture of ice and saline solution. As the temperature falls this solution deposits more and more ice, and its volume increases. But the increase of volume is due to the formation of ice out of water, and not to the expansion of a crystalline solid already formed.

In Table IX. are given the volumes occupied by the ice (with inclosed brine) formed by freezing 100,000 c.c. (at 0° C.) of a water containing chloride of sodium equivalent to 7 grammes chlorine in 1,000,000 cubic centimetres (at 0° C.).

TABLE IX.—*Water containing* 7 *parts Cl in* 1,000,000.

Temp. ° C.	Water frozen. c.c.	Ice formed. c.c.	Brine remaining. c.c.	Ice and Brine. c.c.	Pettersson III. Vol. of Ice at T°. c.c.	Diff.
T	V_1	v_1	V_2	v_2	P	$P - v_2$
−0·07	99000	107979	1000	108979	108980	1
−0·10	99300	108306	700	109006	109007	1
−0·15	99533	108561	467	109028	109038	10
−0·20	99650	108687	350	109037	109048	11
−0·40	99825	108879	175	109054	109057	3

The volume (v_2) of the ice and brine formed on freezing this water is compared with that (P) observed by Pettersson in freezing a sample of the distilled water in ordinary use in the laboratory.

It will be seen that the volumes observed by Pettersson agree very closely with those calculated for a water containing 7 parts of chlorine in a million, on the assumption that the saline matter is contained entirely in adhering liquid brine.

The irregularities in the melting-points of bodies like acetic acid, to which Pettersson refers, are without doubt due to a perfectly similar cause.

Also the very low latent heat observed by Pettersson for sea-water is to be explained by the fact that the salt retains a considerable proportion of the water in the liquid state even at temperatures many degrees below the freezing-point of distilled water.

Thus, he made two determinations of the latent heat of sea-water containing 1·927 per cent. Cl and 3·53 per cent. salt. The freezing took place in the one case between the temperatures −9°·0 and −7°·47 C., and in the other between −8°·35 and −6°·94 C., and the results he found were 52·7 and 51·5. The mean initial temperature in these two experiments is −8°·7 C., and the mean final temperature −7°·2 C. At −7°·2 C. ice would form on cooling, and would melt on warming a solution of chloride of sodium containing 6·48 per cent. Cl, which represents 11·87 per cent. of the sea salt. In order to concentrate a brine containing 3·53 per cent. salt to one containing 11·87 per cent., 70 per cent. of the water in it must be removed. Hence in sea-water freezing at a final temperature of $-7°·2$ C. there is formed 70 per cent. of ice, and there remains liquid 30 per cent. of brine. Freezing began at the mean temperature $-8°·7$ C., and the latent heat of pure ice at this temperature is 75. Calculating the latent heat of this mixture from the heat liberated in the calorimeter during freezing, and assuming that the whole mass had solidified, Pettersson's results give the mean latent heat of this sea-water as 52·1. Calculating the apparent latent heat on the assumption that 70 per cent. of the mass solidifies into pure ice and that 30 per cent. remains liquid, we get the number 51·5. On all grounds therefore we must conclude that pure ice is the primary product in freezing sea-water and saline solutions of moderate concentration.

The *plasticity* of ice and the motion of glaciers receive a simple and natural explanation when we see, as in Table IX., that, if the water from which this ice is produced contains no more than 7 parts of chlorine per millon, it will, in the process of thawing, when the temperature has risen to $-0°·07$ C., consist to the extent of 1 per cent. of its mass of liquid brine or water. The water considered in Table IX. is certainly not less free from foreign ingredients than rain or snow. It follows, therefore, that a glacier, in a climate where the temperature is for the greater part of the year above 0° C., must have a tendency to *flow*, owing to the power of saline solutions to deposit ice and to dissolve it at temperatures below 0° C.

The *verification of thermometers* by comparison with the air thermometer is always troublesome. It results from the above investigations that, if the temperature at which ice melts in solutions of a salt, such as chloride of calcium of different degrees of concentration, were once and for all carefully determined by means of a standard air thermometer, a thermometer could be indirectly but satisfactorily compared with the air thermometer at temperatures below 0° C. by immersing it in a mixture of ice and chloride of calcium solution, and taking a series of readings of the thermometer and samples of the brine simultaneously. By determining the chlorine in the samples the concentration of the brine is ascertained, and the comparison with the standard effected.

Freezing Mixtures.—The results obtained in examining the melting-point of ice in saline solutions affords data for mixing freezing baths of any degree of cooling power. With chloride of sodium, for instance, a rough rule is to have such an amount of salt dissolved in the brine that the percentage of chlorine shall give the desired temperature in Centigrade degrees below the freezing-point. In my experiments in freezing sea-water in quantities of 300 grammes, I usually made up the bath of 500 grammes pounded ice, 400 grammes water, and 45 grammes common salt. When mixed, the liquid contained about 4 per cent. Cl, and gave a temperature a little below $-4°$ C. In the course of an hour the liquid would contain 3 per cent. to 3·25 per cent. Cl, and the temperature have risen to $-3°$ C. By using such baths freezing operations can always be kept completely in hand.

3. On the Distribution of Temperature in the Antarctic Ocean.

By J. Y. Buchanan.

(*Abstract.*)

In the regions of the Antarctic Ocean where icebergs are numerous, and where in winter the sea-water freezes, the distribution of temperature in the deeper layers of water is peculiar. The facts are detailed in the *Challenger Narrative* (vol. i.). The general result of her observations went to show that, from the edge of the ice-pack, a wedge of cold water stretches northwards for more than 12° of latitude, underlying and overlying strata at a higher temperature than itself (p. 418).

Although the conditions and facts likely to throw light upon the cause of this phenomenon are discussed, no satisfactory explanation of it is given. One important fact is noticed at p. 421—"The fact that the cold wedge above referred to extended north just as far as the icebergs did in March 1874 points to there being some connection between the temperature and the presence of melting icebergs." It is well known that icebergs consist of land-ice, which is nearly pure frozen water, and melts in the air at 32° F. It was thought that the effect of immersion of such a substance in a medium having a temperature 3° F. lower than its melting-point would be to indefinitely preserve it—that, in fact, only the lower

surfaces of the icebergs large enough to reach to the depth of 300 fathoms would suffer any melting at all. The existence of the cold stratum was ascribed wholly to the cold brine, separated from the ice on the freezing of the sea-water, sinking downwards with an initial temperature of from 28°·5 to 29° F. This cause, though existing and in operation, is quite inadequate to produce the effect observed. The facts related in the preceding paper furnish a complete explanation of the cold wedge of water in the Antarctic Ocean and the dependence of its thickness and temperature on the range of icebergs. These enormous islands of ice, a very large proportion of which rise in tabular form to a height of 200 to 300 feet above the sea, float in many cases with their lower surfaces at a depth of from 250 to 300 fathoms. The warmer and denser water coming from lower latitudes (see *Challenger Narr.*, vol. i. p. 428) bathes these lower surfaces, the temperature of the mixture at the surface of contact falls, the heat abstracted from the sea-water melts a corresponding amount of the ice of the iceberg, and a saline solution is produced, less salt, and therefore lighter than the water away from contact with the iceberg, and having a temperature which depends immediately on the strength of the resulting solution. Being lighter than the surrounding water, this resulting solution necessarily flows up along the sides of the berg to the surface, and its place is taken by fresh undiluted sea-water, which in its turn is cooled, diluted, and transferred to the surface. The result is the production of a means of circulating and of cooling and equalising the temperature of the water within the reach of icebergs. As there is continual renewal of the ocean water brought into contact with the ice, and as its composition is constant, the temperature produced is practically constant, namely, 28°·8 to 29°·0 F., or – 1°·7 to – 1°·8 C. The layer of lighter water from 50 to 80 fathoms thick at the surface is due principally to this melting of land-ice, though it is also due in small proportion to the melting of sea-ice.

Table giving the Temperature at which Ice melts in Sea-Water containing different percentages of Chlorine.

Temp. C., . .	1°·0	1°·1	1°·2	1°·3	1°·4
Per cent. Cl, . .	1·040	1·131	1·222	1·313	1·404
Temp. C., . .	1°·5	1°·6	1°·7	1°·8	1°·9
Per cent. Cl, . .	1·495	1·586	1·678	1·769	1·880

The density (at 15°·56 C.) of the sea-water which comes in contact with the lower surfaces of the icebergs is 1·0255, which represents a chlorine percentage of 1·90. Ice actually *melting* in this water would produce a temperature of – 1°·92 C. When ice is *immersed* in this water it lowers its temperature, and a portion of the ice is melted, producing dilution. The concentration, therefore, or chlorine percentage, which will determine the melting temperature of the ice, will be a little lower than that of the original sea-water. From the "Challenger" observations we see that, on the confines of the pack ice, the cold stratum of water has a uniform temperature of 29° F. (– 1°·67 C.). Ice melts at this temperature in sea-water containing 1·65 per cent. of chlorine. In this process ice is melted, so that 100 grammes pure warm sea-water become 119 grammes of diluted cold sea-water. It will be observed that the ice which has been formed in the atmosphere at a temperature of 32° F. comes in this way to be melted at a temperature of 29° F.; and the pressure exerted by the 300 fathoms of sea-water, though it may assist in the lowering of the melting temperature, is insufficient to account for the amount.

Monday, 4th April 1887.

Dr J. MURRAY, Vice-President, in the Chair.

The following Communications were read:—

1. Note on a Formula for $\Delta^n 0^i / n^i$ when n, i are very large Numbers. By Professor A. Cayley.

The following formula

$$\frac{\Delta^n 0^i}{n^i} = e^{-nq}\left[1 + \left(\frac{i+1-2n}{2n}\right)q - \left(\frac{n+i+2}{2}\right)q^2\right], \quad q = e^{-(i+1)/n}$$

is given by Laplace (*Theorie Analytique des Probabilités*, 2 ed. Paris, 1814, p. 195) as an approximate value of $\Delta^n 0^i / n^i$, when n and i are very large numbers, and is applied immediately afterwards to the case where i is of the order $n \log n$. As remarked by Professor

Tait, it is certainly not applicable to the case where i is of the order n, for taking $i = An$, A a given number however large, then q is indefinitely near to the very small value e^{-A}, but nevertheless the last term $-\frac{1}{2}(n+i+2)q^2$, by taking n sufficiently large may be made as large as we please, and the value would thus come out negative. It is thus necessary that i should be at least of the order $n \log n$; but it may be of any higher order.

Writing for greater convenience $r = ne^{-i/n}$ (where r is not very large) then $nq = re^{-1/n} = r(1-X)$ if $X = 1 - e^{-1/n}$, and the formula becomes

$$\frac{\Delta^n 0^i}{n^i} = e^{-r(1-X)}\left[1 + \frac{i+1-2n}{2n} e^{-1/n}\frac{r}{n} - \frac{n+i+2}{2} e^{-2/n}\frac{r^2}{n^2}\right].$$

Here $X = \frac{1}{n} - \frac{1}{1.2}\frac{1}{n^2} + \frac{1}{1.2.3}\frac{1}{n^3} + \&c.$, and the exponential e^{rx}

$= 1 + rX + \frac{r^2X^2}{1.2} + \ . \ . \ .$ is thus also expansible in negative powers of n, and the formula becomes

$$\frac{\Delta^n 0^i}{n^i} = e^{-r}\left(1 + rX + \frac{r^2X^2}{1.2} + \ . \ . \ .\right)\left[1 + \frac{i+1-2n}{2n} \, . \, e^{-1/n}\frac{r}{n} - \frac{n+i+2}{2}e^{-2/n}\frac{r^2}{n^2}\right]$$

viz., putting for X its value,

$$\begin{aligned} &= e^{-r}\{1 \\ &+ r \, . \left(\frac{i+1-2n}{2n^2} e^{-1/n} + 1 - e^{-1/n}\right) \\ &+ r^2\left(\frac{-n-i-2}{2n^2} e^{-2/n} + \frac{i+1-2n}{2n^2}\left(1 - e^{-1/n}\right)e^{-1/n} + \frac{1}{2}\left(1 - e^{-1/n}\right)^2\right) \\ &+ \&c.\} \end{aligned}$$

or finally expanding the $e^{-1/n}$, and taking the whole result as far as $\frac{1}{n^2}$: coefficient of r is

$$\left(-\frac{1}{n} + \frac{i+1}{2n^2}\right)\left(1 - \frac{1}{n}\right) + \frac{1}{n} - \frac{1}{2n^2}, \ = \frac{1+\frac{1}{2}i}{n^2};$$

coefficient of r^2 is

$$\left(-\frac{1}{n} + \frac{-2-i}{n^2}\right)\left(1 - \frac{2}{n}\right) + \left(-\frac{1}{n} + \frac{4i}{2n^2}\right)\frac{1}{n} + \frac{1}{2n^2}, \ = -\frac{\frac{1}{2}}{n} + \frac{-\frac{1}{2} - \frac{1}{2}i}{n^2};$$

whence the formula becomes

$$\frac{\Delta^n 0^i}{n^2} = e^{-r}\left\{1 + r\frac{1+\frac{1}{2}i}{n^2} + \frac{r^2}{1.2}\left(-\frac{1}{n} + \frac{-1-i}{n^2}\right) + ..\right\}.$$

It seems to me that the correct result up to this order of approximation is

$$\frac{\Delta^n 0^i}{n^2} = e^{-r}\left\{1 + r\frac{\frac{1}{2}i}{n^2} + \frac{r^2}{1.2}\left(-\frac{1}{n} + \frac{-i}{n^2}\right)\right\}.$$

My investigation is as follows: we have

$$\frac{\Delta^n 0^i}{n^i} = 1 - \frac{n}{1}\left(1-\frac{1}{n}\right)^i + \frac{n\,.\,n-1}{1\,.\,2}\left(1-\frac{2}{n}\right)^i + \ldots$$

the series being a finite one, but the number of terms is very large. But observe that, however large n is, we can take i so large that the second term $n\left(1-\frac{1}{n}\right)^i$ may be as small as we please; taking this term to be of moderate amount, say $= r_1$, the subsequent terms will be not very different from $\frac{r_1^{\,2}}{1\,.\,2}, \frac{r_1^{\,3}}{1\,.\,2\,.\,3}, \ldots$ and the approximate value is $1 - r_1 + \frac{r_1^{\,2}}{1\,.\,2} -$ &c., which is a convergent series having its sum $= e^{-r_1}$. To work this properly out, I represent the successive terms by $r_1, \frac{r_2}{1\,.\,2}, \frac{r_3}{1\,.\,2\,.\,3}, \ldots$ so that the series is

$$= 1 - r_1 + \frac{r_2}{1\,.\,2} - \frac{r_3}{1\,.\,2\,.\,3} + \ldots$$

Taking r a value at pleasure not very different from r_1, and multiplying by

$$(1=)e^{-r}.\,e^{r} = e^{-r}.\left(1 + r + \frac{r^2}{1\,.\,2} + \ldots\right),$$

the sum is

$$= e^{-r}.\left\{1 + (r - r_1) + \frac{1}{1\,.\,2}(r^2 - 2rr_1 + r_2)\right.$$

$$\left. + \frac{1}{1\,.\,2\,.\,3}(r^3 - 3r^2r_1 + 3rr_2 - r_3) + \ldots\right.$$

Assume now $r = ne^{-i/n}$, we have

$$r_1 = n\left(1-\frac{1}{n}\right)^i = ne^{i\log\left(1-\frac{1}{n}\right)} = r(1+\text{X}_1);\ \text{X}_1 = e^{-\frac{1}{2}\frac{i}{n^2} - \frac{1}{3}\frac{i}{n^3} - ..}$$

and similarly

$$r_2 = n\,.\,n-1\,.\left(1-\frac{2}{n}\right)^i = n^2\left(1-\frac{1}{n}\right).\,e^{i\log\left(1-\frac{2}{n}\right)}$$

$$=\left(1-\frac{1}{n}\right)r^2(1+\mathrm{X}_2); \qquad \mathrm{X}_2 = e^{-\frac{1}{2}\frac{4i}{n^2}-\frac{1}{3}\frac{8i}{n^3}-\cdots}$$

$$r_3 = n\,.\,n-1\,.\left(1-\frac{2}{n}\right)^i = n^2\left(1-\frac{1}{n}\right).\,e^{i\log\left(1-\frac{2}{n}\right)}$$

$$=\left(1-\frac{1}{n}\right)\left(1-\frac{2}{n}\right)r^3(1+\mathrm{X}_3); \qquad \mathrm{X}_3 = e^{-\frac{1}{2}\frac{9i}{n^2}-\frac{1}{3}\frac{27i}{n^3}-\cdots}$$

$$\vdots \qquad\qquad \vdots \qquad\qquad \vdots$$

It is now easy to calculate the successive terms $r-r_1$, $r^2-2rr_1+r_2$, &c., and it is to be observed that, in the parts independent of the X's, we have only terms divided by n, n^2, or higher powers of n: thus in $r^4-4r^3r_1+6r^2r_2-4r^3r_3+r_4$ we have r^4 into

$$1-4+6\left(1-\frac{1}{n}\right)-4\left(1-\frac{1}{n}\right)\left(1-\frac{2}{n}\right)$$
$$+\left(1-\frac{1}{n}\right)\left(1-\frac{2}{n}\right)\left(1-\frac{3}{n}\right), \quad =\frac{3}{n^2}-\frac{6}{n^3}.$$

We thus obtain the formula

$$\begin{array}{llll}
\dfrac{\Delta^n 0^i}{n^i} = e^{-r}\Big\{1 & & & \\
+ & r & \Big(& -1\mathrm{X}_1\Big) \\
+ & \dfrac{r^2}{1.2} & \Big(-\dfrac{1}{n} & -2\mathrm{X}_1+\left(1-\dfrac{1}{n}\right)\mathrm{X}_2\Big) \\
+ & \dfrac{r^3}{1.2.3} & \Big(-\dfrac{2}{n^2} & -3\mathrm{X}_1+3\left(1-\dfrac{1}{n}\right)\mathrm{X}_2-\left(1-\dfrac{1}{n}\right)\left(1-\dfrac{2}{n}\right)\mathrm{X}_3\Big) \\
+ & \dfrac{r^4}{1.2.3.4} & \Big(-\dfrac{3}{n^2}-\dfrac{6}{n^3} & -4\mathrm{X}_1+6\left(1-\dfrac{1}{n}\right)\mathrm{X}_2-4\left(1-\dfrac{1}{n}\right)\left(1-\dfrac{2}{n}\right)\mathrm{X}_3 \\
 & & & +\left(1-\dfrac{1}{n}\right)\left(1-\dfrac{2}{n}\right)\left(1-\dfrac{3}{n}\right)\mathrm{X}_4\Big) \\
 & & \vdots & \vdots \qquad\qquad \vdots\Big\}.
\end{array}$$

where $r=ne^{-i/n}$ as above, and X_1, X_2, ... have the above-mentioned values, the exponentials being expanded in negative powers of n.

Writing $X_1 = \frac{-\frac{1}{2}i}{n^2}$, $X_2 = \frac{-2i}{n^2}$ we have

$$\frac{\Delta^n 0^i}{n^2} = e^{-r}\left\{1 + r\frac{\frac{1}{2}i}{n^2} + \frac{r^2}{2}\left(-\frac{1}{n} + \frac{-i}{n^2}\right)\right\},$$

which is the foregoing approximate value.

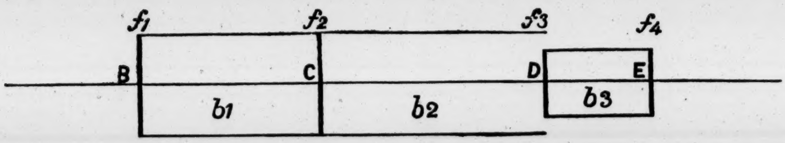
2. On the Fossil Flora of the Radstock Series of the Somerset and Bristol Coal Fields. (Upper Coal Measures.) Part I. By R. Kidston, Esq., F.G.S.

3. On the Achromatism of the Four-Lens Eye-Piece: New Arrangement of the Lenses. By Edward Sang, LL.D.

In designing a telescope for a particular class of observations, it was found desirable to have a field-bar in the focus of the object-glass, and, at the same time, to have an image of that object-glass exterior to the eye-lens. These desiderata cannot both be got by the achromatic arrangement of two lenses made of one material; they are combined in the ordinary four-lens eye-piece.

While investigating the action of the four lenses with a view to a third condition, found, however, to be unattainable, I was led to notice a porism altogether new to me, and which guides us to a new arrangement of the lenses. Believing that this porism has hitherto escaped notice, I venture to submit it to the Society.

Let B, C, D, E represent four thin lenses, all of one material, arranged along the axis of a telescope, whose object-glass A is at a distance beyond the limits of the page, and let us denote their focal lengths by f_1, f_2, f_3, f_4,



while their distances are b_1, b_2, b_3; then the condition of achromatism is contained in the equation

$$\begin{aligned}0 = {} & f_1 f_2 f_3 + f_1 f_2 f_4 + f_1 f_3 f_4 + f_2 f_3 f_4 - 2b_1(f_2 f_3 + f_2 f_4 + f_3 f_4) \\ & - 2b_2(f_1 + f_2)(f_3 + f_4) - 2b_3(f_1 f_2 + f_1 f_3 + f_2 f_3) \\ & + 3b_1 b_2(f_3 + f_4) + 3b_1 b_3(f_2 + f_3) + 3b_2 b_3(f_1 + f_2) \\ & - 4b_1 b_2 b_3 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(A)}.\end{aligned}$$

With this one condition of achromatism among the seven quantities, we are at liberty to conjoin other conditions, subject of course to their possibility. The most obvious collateral condition is that the combination DE be achromatic in itself, so that it may be used separately as an inverting eye-piece. This achromatism is expressed by the equation

$$0 = f_3 + f_4 - 2b_3 \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(B)},$$

and, on eliminating b_3 thereby, we get

$$\begin{aligned}0 = {} & 2(f_1 + f_2)f_3^2 + b_1(f_2 f_3 + f_2 f_4 + f_3 f_4 - 3f_3^2) \\ & + b_2(f_3 + f_4)(f_1 + f_2 - 2b_1) \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(C)}.\end{aligned}$$

This equation (C) shows that the pair B, C cannot be made self-achromatic at the same time with the pair D, E, because this would imply the condition $f_1 + f_2 - 2b_1 = 0$, and would necessitate an infinite value for b_2. The equation (C) may be written in the form

$$\left\{ \frac{2f_3}{f_2 + f_4}(2b_1 - f_1 - f_2) - b_1 \right\} f_3 = b_1 f_2 + b_2(f_1 + f_2 - 2b_1) \, . \qquad \text{(C)},$$

in which the second member is altogether independent of f_3 and f_4.

Now, in arranging a Huygenian eye-piece, we may assume any ratio between f_3 and f_4. If we wish to use a field-bar, as in surveying instruments, we make f_4 the greater, otherwise we prefer to make f_4 the lesser, because then the image of the object-glass is outside of and close to the lens E, and so a large field of view is had. In this case it is important that the Rheita's lens D be well out of the focus of E, in order that any minute imperfections or dust-particles on its surface may be out of view. One maker may prefer the ratio two to one; another maker may adopt that of three to one.

If then we have fixed upon some ratio n, and resolved to make in all our Huygenian eye-pieces

$$f_3 = nf_4 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(D)},$$

the above equation takes the new form

$$f_3 . \frac{(3n-1)b_1 - 2n(f_1+f_2)}{n+1} = b_1 f_2 + b_2(f_1+f_2-b_1),$$

and f_3 becomes indeterminate when both the conditions

$$(3n-1)b_1 - 2n(f_1+f_2) = 0,$$

and

$$b_1 f_2 + b_2(f_1+f_2-b_1) = 0$$

are satisfied at the same time.

If, therefore, we make

$$b_1 = \frac{2n}{3n-1}(f_1+f_2) \quad . \quad . \quad . \quad . \quad . \quad . \quad (E)$$

and

$$b_2 = \frac{2n}{n+1} f_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (F)$$

we may use, along with these, any eye-piece C, D, provided that it have

$$f_2 : f_4 :: n : 1.$$

An eye-piece constructed in this way has the several advantages belonging to all four-lens ones. It shows the objects in their natural position; it allows of a field-bar across which cobwebs and micrometric scales may be placed; a stop between the lenses B and C, having its aperture equal to the image of the object-glass, cuts off all extraneous light; we may introduce there a sun-screen, which shall not be heated so intensely as that usually placed outside of the eye-lens E. But this particular arrangement superadds another. Our inverting Huygenian eye-pieces having been all constructed to the same ratio n, we screw on the portion B, C, D, or uprighter as it may be called, and are then at liberty to use any one of our battery, the magnifying powers being at the same time considerably augmented.

4. An Effective Arrangement for Observing the Passage of the Sun's Image across the Wires of a Telescope. By Edward Sang, LL.D.

At night the cobwebs of the telescope are invisible for want of light; we have to illuminate either the field or the wires, so as to make them visible by contrast. At noon the astronomer meets with the same kind of difficulty from an opposite cause; the intense sun's light has to be obscured by a dark glass, which, at the same time, completely obliterates the spider-lines; these are only seen on the sun's face. In consequence, the advent of the sun's edge to a wire cannot be observed; the line must be fairly on the sun's face before we can see it, and thus the noted instant is necessarily too late,—too late by a quantity depending on the power of the telescope and on the skill of the observer. Hence the estimate of the sun's apparent diameter from observations of the meridian passage may be expected to err slightly in defect, while the thence-deduced right ascension must be too great.

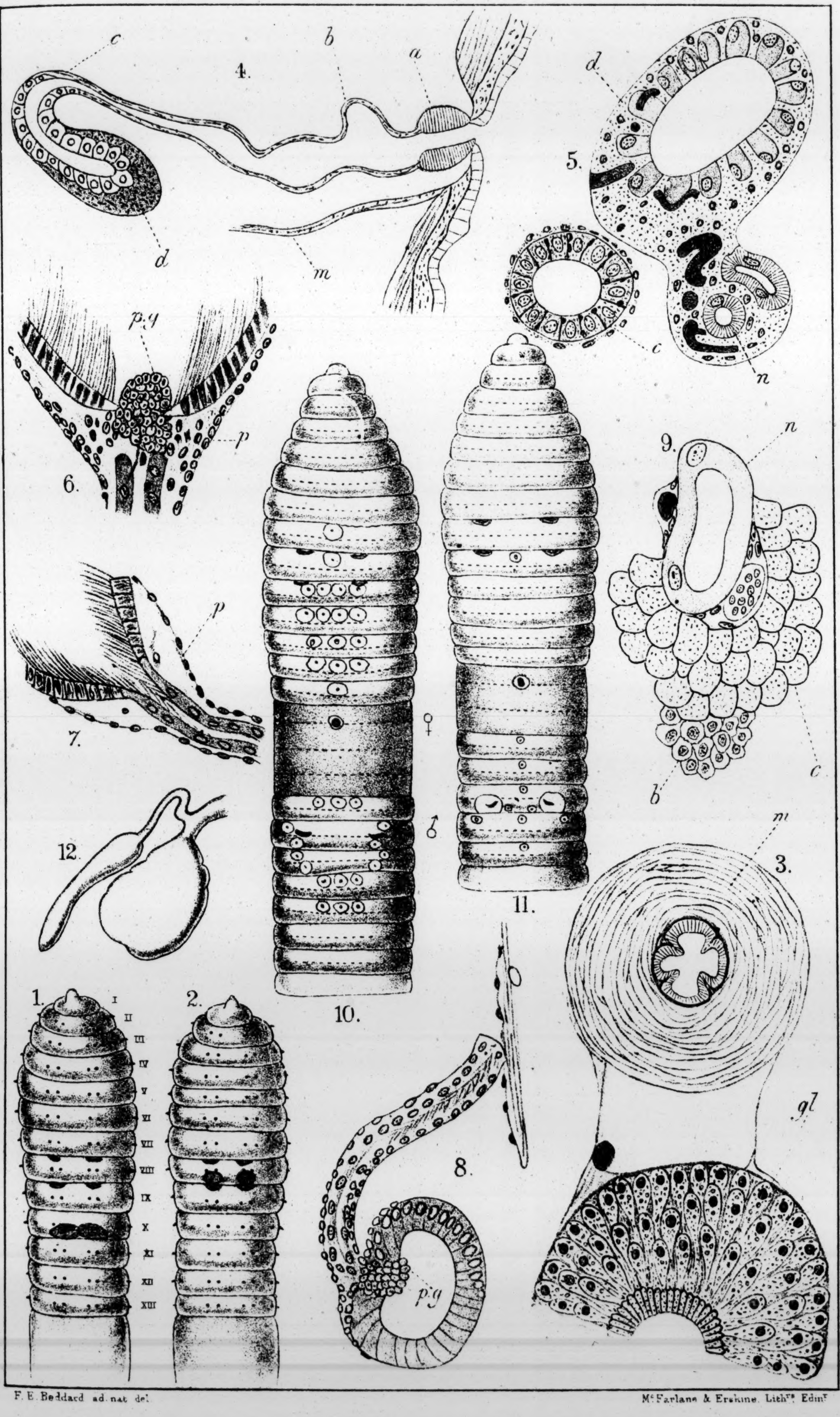
But, if a thin cloud pass before us, we use a paler screen and see the wires over the whole field while the sun's edge remains distinctly defined; the observations are then satisfactorily made. It occurred to me that, at all times, we may make an artificial cloud, and, to-day just four weeks ago, I laid a thin muslin over the object-glass of my alt-azimuth, and got all that is needed.

5. Observations on the Structural Characters of certain new or little-known Earthworms. By Frank E. Beddard, M.A., Prosector to the Zoological Society of London, and Lecturer on Biology at Guy's Hospital. (Plate V.)

The present paper contains a description of five apparently new species of Lumbricidæ from Australia and New Zealand, one of these species being perhaps the type of a new genus, which I have named *Neodrilus;* the remaining species are *Acanthodrilus neglectus*, from New Zealand, *Perichæta newcombei*, *Urochæta*, sp.?, from Australia, and *P. upoluensis*, from one of the Pacific islands. I have endeavoured to make these descriptions as full as the material, in many cases in an excellent state of preservation, has enabled me to do. I have also incorporated into this paper some few notes on *Perichæta antarctica*, Baird, a species which has not yet been sufficiently discriminated.

Acanthodrilus neglectus, n. sp.

In my paper on New Zealand Lumbricidæ, recently published in the "Proceedings of the Zoological Society" (*P. Z. S.*, 1885, pt. iv.), Proc. Roy. Soc. Edinr. Vol. XIV, Plate V.



F. E. Beddard ad nat. del.

McFarlane & Erskine, Lithrs. Edinr.

I described two species of *Acanthodrilus*—*A. novæ-zelandiæ* and *A. dissimilis*—very closely allied in structure, and agreeing in a number of points to differ from the third species, *A. multiporus*. *A. dissimilis* is distinguished from *A. novæ-zelandiæ* mainly by the character of the spermathecæ; these organs are present in both species to the number of two pairs. In *A. novæ-zelandiæ* each spermatheca, which is somewhat pear-shaped, is provided with a number of small diverticula arranged round its external orifice; in *A. dissimilis*, the spermatheca has but a single pair of diverticula, which are of very considerable size. The former species is also frequently provided with a double dorsal blood-vessel; this character is, however, not absolutely distinctive of *A. novæ-zelandiæ*; some individuals agree with *A. dissimilis* in possessing a single dorsal vessel. I may state that the condition of the dorsal vessel is no criterion of the age of the individual. In the largest specimens of *A. novæ-zelandiæ* dissected by me the dorsal vessel was double, while those specimens in which it was represented by a single tube happened to be very small.

On again looking through the collection of New Zealand earthworms which Prof. T. J. Parker kindly sent me, I find that I have confounded two apparently distinct species under the name of *Acanthodrilus dissimilis*.

As there are a large number of individuals of *A. dissimilis* which fall into two series, I think that I am justified in making a specific, or at least a subspecific distinction, although the point wherein the two series of individuals differ is after all rather a small one; but it seems to me that a differential character, if it be constant for a large number of specimens, is of importance, however small.

The accompanying drawings illustrate the difference to which I refer.

In fig. 1, which represents the anterior segments of the body seen ventrally, there are a pair of genital papillæ situated on segment 10. For this variety I shall retain the name *A. dissimilis*. In fig. 2 the genital papillæ occupy a different position; they are situated on the 8th segment. For this variety I propose the name of *A. neglectus*.

Neodrilus monocystis, nov. gen. et sp.

On looking over a collection of earthworms which I have received from New Zealand by the kindness of Prof. T. J. Parker, I found a single individual which differs from the rest in a number of characters. The remaining specimens belong to three distinct new species which I have lately described,* referring them to the genus *Acanthodrilus.* The specimen which forms the subject of the present communication appeared at first sight to belong to the species *Acanthodrilus dissimilis*, F. E. B., though considerably more slender than any of the individuals of that species which the collection contained.

The setæ are disposed in four series of pairs, and the nephridial pores alternate in position precisely as in *Acanthodrilus dissimilis.* The clitellum occupies a similar position, and extends over an equal number of segments, viz., 5 (13–17). Instead of there being two pairs of spermathecal apertures, there is only a single one situated between the 7th and 8th segments, on a line with the inferior pair of setæ. The male generative pore is placed upon the 17th segment, and each pore is continuous with a groove upon the integument, which extends over the following segment, and ends upon the middle of the 19th segment. I could not, however, detect a second pair of male generative pores upon this segment. In *Acanthodrilus dissimilis*—at least in many individuals—there is a similar groove connecting the genital pores of the 17th with those of the 19th segment entirely similar to that of *Neodrilus.* It is possible that this supposed new genus *Neodrilus* is really an *Acanthodrilus*, in which the posterior pair of male generative pores, together with their glands, have not yet been developed. I am not aware, however, of any similar instance in the genus *Acanthodrilus*, and the present species is fully mature. In favour of this supposition, however, is the condition of certain other peculiar accessory generative structures which I have lately described † in a species of *Acanthodrilus* from New Caledonia ; these are sometimes present and sometimes absent in mature individuals. Another possibility is that the present individual is abnormal, and it is principally for these reasons that I have hesitated in making a new genus ; though there can be no doubt of the *specific* distinctness of the worm.

The male generative pore is continuous with a long, coiled, tubular prostatic gland, the proximal region of which is a slender muscular (fig. 3 *m*) tube, while the distal region is thick and glandular (fig. 3 *gl*); with the aperture is also connected a thin-walled sac containing a bundle of long penial setæ.

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* *Proc. Zool. Soc.*, 1885, pt. iv.

† *Proc. Zool. Soc.*, 1886.

Spermathecæ.—In the 8th segment are a pair of oval spermathecæ, which open on to the exterior in the groove which separates this segment from the one in front. As is so generally the case, they are provided with a diverticulum. The diverticulum of each spermatheca lies in the segment in front of that which contains the spermatheca itself, and is remarkable in being actually larger than the spermatheca. In most, if not in all, the genera of earthworms which are included in Perrier's two groups, Intraclitellians and Postclitellians, the spermathecæ open close to the anterior boundary of the segment which contains them. In certain species of *Lumbricus* and other Anteclitellian genera, the position is sometimes different, the spermathecal apertures being situated near to the posterior boundary of their segment. In one instance, at any rate, the spermathecæ actually perforate the mesentery bounding the segment which contains them on their way to the exterior. In a species of earthworm lately described by myself in a note communicated to the Royal Society of Edinburgh,* this is the case with one or more of the seven pairs of spermathecæ which are present in that species. It might be imagined, therefore, that in *Neodrilus* the anterior larger portion of the spermatheca really corresponds to the spermatheca, while the posterior smaller portion is the homologue of the diverticulum so constantly found in *Perichæta*, *Acanthodrilus*, and in other genera. Without an examination of the minute structure of the two regions of the spermatheca, it would be difficult to say which was spermatheca and which diverticulum. In three species of *Acanthodrilus* I have described, I believe for the first time, a very marked difference in minute structure between the spermatheca and the diverticulum, which is correlated with the fact that the spermatozoa always appear to be stored up in the diverticula. In the present species I find an identical difference in the structure of the spermatheca and its appendage, which leads to the inference that the anterior sac is the diverticulum. Seeing that in many cases, especially in *Perichæta*, the diverticula of the spermathecæ extend into the segment anterior to that which contains the spermatheca itself, the disposition of these structures in *Neodrilus* is perfectly normal.

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* *Proc. Roy. Soc. Edin.*, 1885–6, p. 451.

The *Nephridia* have exactly the same structure as in *Acanthodrilus dissimilis*, and, as already mentioned, alternate in position from segment to segment in the same fashion. This fact cannot, however, be regarded as a proof that the two worms belong to the same genus. I shall have occasion to point out in a future paper that an Australian earthworm, *Cryptodrilus fletcheri*, n. sp., possesses nephridia which are in every respect similar to those of *Acanthodrilus* and *Neodrilus* in structure and in position, and other instances are there mentioned.

Urochæta, sp.

The present description is the outcome of an investigation into the structure of an Australian species of the genus *Urochæta*. The specimens were kindly given to me by Mr S. Prout Newcombe, and come from Queensland. I have been able to examine a large number, all of which were in a very fair state of preservation for microscopical examination.

The genus is at present known to inhabit Brazil, the West Indies, Java, Sumatra, and Australia, and comprises only three species at most. The first of these was originally described by Fritz Müller,* who met with it in Brazil, under the name of *Lumbricus corethrurus*. The specific name "brush-tail" was given to the worm on account of the irregular disposition of the setæ at the posterior end of the body; the segments are in this region of the body very close together, and the setæ being usually (at least in the contracted state of the body) much protracted and directed backwards, the aptness of the name will be very evident to any one acquainted with these worms. Fritz Müller did not thoroughly investigate the structure of the worm, and was therefore unable to see any reason for removing it from the genus *Lumbricus*.

A somewhat fuller account of the same species was given by Perrier in his "Recherches pour servir à l'histoire des lombriciens serrestres." † Perrier rightly created a new genus for the reception of this species, which he termed *Urochæta,* but he altered the specific name of Müller into "*hystrix.*" Two years later M. Perrier published * a much more detailed and beautifully illustrated memoir upon the same species, which he referred to more correctly under the name of *Urochæta corethrura.* The specimens investigated by Perrier were obtained, not only from Brazil, but also from the West Indies (Martinique), and, which is more remarkable, from the island of Java. Perrier is inclined to think that the occurrence of the same species in the New World and in Java is rather to be explained by its accidental importation into the latter country, than to be regarded as of importance as a fact in geographical disposition. The occurrence, however, of a very closely allied species in the neighbouring island of Sumatra is somewhat against the supposition, and I am not at all certain that the species to be described in the present paper—a native of Australia—is really different from *Urochæta dubia.*

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* *Arch. f. Naturg.*, xxiii.; *Ann. and Mag. Nat. Hist.*, ser. 2, vol. xx.; *Abh. d. naturf. Gesellsch. in Halle*, v., vi., 1857; in *Landplanarien*, von Max Schultze.

† *Nouv. Arch. d. Muséum*, t. viii. (1872).

A second species of the genus has been quite lately described by Dr Horst, † under the name of *Urochæta dubia,* from Sumatra. Dr Horst's description is necessarily—owing to the poor condition of his material—brief, and only refers to the more important points.

The differences between *Urochæta dubia* and *U. corethrurus* are chiefly in the position of the spermathecæ (situated in segments 6, 7, 8, instead of 8, 9, 10) and in the fact that there are four pairs of modified clitellar setæ, a pair upon each of the segments 18, 19, 20, 21, instead of the single pair (on segment 20) of *U. corethrurus.* It appears also that in Horst's species all the segments anterior to the clitellum are furnished with setæ, while in *U. corethrura* the first three segments are devoid of these structures; furthermore, the irregularity in the arrangement of the setæ begins to be evident in segment 10 in *U. dubia,* and not until segment 14 in *U. corethrurus.*

The *clitellum* is very readily to be made out with specimens of the Australian *Urochæta,* and occupies about eight segments, commencing with the 14th and ending with the 22nd. Very often the first and last of these segments were only partially

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* *Arch. de Zool. Exp.,* t. iii. (1874).

† *Midden Sumatra,* Vermes door Dr R. Horst, p. 7.

invaded by glandular substance. In fully mature individuals the clitellum was perfectly developed on the ventral as well as on the dorsal side of the segments pertaining to it. A remarkable fact about the clitellum of this species is that the glandular substance is entirely undeveloped between the segments, so that this region of the body is just as plainly segmented as any other region; indeed, the contrast between the thick glandular appearance of the segments themselves, and the deep furrows which separate them, renders the segmentation if anything rather more conspicuous than elsewhere.

It is to be noted that the number of segments occupied by the clitellum and their position is the same as in the other two species of *Urochæta*.

The disposition of the *setæ* is remarkable; in the anterior segments of the body, comprising the first eight segments, the setæ are arranged, as in *Lumbricus*, in four series of pairs; the two setæ of each pair are closely approximated to each other, and the intervals between the pairs are not widely different.

In the 9th segment there is already some little difference in the setæ; the two setæ of each of the ventral pairs are at a little greater distance from each other than in the preceding segments; the dorsal pair of setæ of the right side is completely similar to the same pair of setæ in the foregoing segments; on the left side, however, the two setæ have become widely separate, the distance between them being much greater than that which separates the individual setæ of the ventral pairs.

In the next segment the two setæ of each of the ventral pairs are somewhat more widely separated from each other, but the two setæ of each of the dorsal pairs are again quite close together, as in the earlier segments.

In the next few segments the two ventral pairs of setæ remain exactly as in the segments just described; the innermost setæ of the dorsal pairs correspond exactly in position to the innermost of the same pair of setæ in the earlier segments. The outermost setæ, however, vary very much in position, being sometimes nearer to, and sometimes further away from the innermost setæ; moreover, the two halves of the body are not symmetrical in this respect.

Throughout the greater part of the body, commencing shortly after the clitellar segments, if not earlier, the setæ have a partly regular, partly irregular arrangement. The ventral setæ of each pair have a fixed position, and correspond for a large number of consecutive segments; the dorsal setæ of each pair are, on the contrary, quite irregular in their disposition. There appears to be no regular alternation in their arrangement; it sometimes happens that the seta of two consecutive segments will correspond in position, sometimes the setæ of one segment, and the next but one or next but two, &c., that it is impossible to lay down any general statements. The two halves of the body are not symmetrical in respect of their setæ. In the hinder part of the body there is a perfectly regular alternation of the setæ from segment to segment; each seta of one segment is exactly between two setæ of the preceding and consecutive segments; and this statement applies to all the setæ in that region of the body, hence there are exactly sixteen rows in this region of the body, while there are a great many more anteriorly.

In *U. corethrurus* the setæ of the anterior segments are disposed regularly and in pairs; but the two setæ of each pair do not appear from Perrier's description to be so closely applied as in my species. They agree in the fact that in the posterior part of the body the setæ regularly alternate, each seta being placed between two setæ of the preceding and succeeding segments. Perrier, however, says nothing about the disposition of the setæ in the middle portion of the body. I must assume, therefore, for the present that the remarkable arrangement of the setæ of my *Urochæta* in this, by far the greater portion of the body, is peculiar to that species, and distinguishes it from *Urochæta corethrurus.* Dr Horst's description of *Urochæta dubia* seems to show that this species differs but little in this particular respect from *U. corethrurus.*

With regard to the shape of the setæ, I have to record an important difference from *U. corethrurus.* Perrier describes and figures the setæ in the latter species as being bifid at their free extremity, and dwells upon the similarity in this respect to the *Naidea.* Horst says nothing about the structure of the setæ in *U. dubia.* In my species I did not succeed in observing any bifurcation of the distal extremity of the setæ; these structures are, in fact, precisely similar to those of other earthworms. This difference might be regarded as of generic value, were it not for the correspondence in all other essentials of structure.

Another point of difference from *U. corethrurus* concerns the genital setæ; not, however, in their general shape, for I find no difference in this respect between the genital setæ of my *Urochæta* and those figured by Perrier. But while in *U. corethrurus* the genital setæ are confined to segment 20, where they replace the ventralmost setæ on either side, my species has four pairs of these peculiarly modified setæ; they have precisely the distribution mentioned by Horst in *U. dubia*, being found upon segments 18–21, and occupying the position of the ventralmost setæ.

Intersegmental Septa.—As in so many other species and genera of earthworms, the present species exhibits a thickening of certain of the anterior mesenteries. There are four of these specially thickened mesenteries, the first of which immediately follows the gizzard; the last forms the posterior boundary of segment 10. It is in the segments bounded by these thick mesenteries that the spermathecæ lie.

The hindermost of these thickened mesenteries, as already stated, marks off the 10th from the succeeding segment; the arrangement of the mesenteries in front of this does not correspond exactly with the external segmentation. The posterior spermatheca lies in a segment which is bounded anteriorly by the last but one of the thickened mesenteries, and posteriorly by the last of these; externally, however, this segment is distinctly separated by a cross furrow into two segments; and, moreover, the difference between the external and internal segmentation is not only marked by a cross furrow, but also by what is more important, namely, a distinctly double row of setæ.

In the median ventral region of the body there are traces left of the mesentery which should divide the 9th from the 10th segment on either side of the nerve cord; and symmetrically disposed in relation to the nerve cord and to each other is a muscular band, which is attached above to the posterior stout mesentery, and below to the furrow which marks the division between the 9th and 10th segments. The stout mesenteries are everywhere at their insertion on to the body wall divided into separate muscular bands, two of them only being left between segments 8 and 9.

A comparison of the above description with that of Perrier (*loc. cit.*, p. 390) will show that there is some little difference in these points from *U. corethrurus.* Perrier, in fact, states that in his species the specially thickened mesenteries are inserted on to the posterior margin of segments 5, 7, 8, 9, and 11; two segments, viz., 6 and 10, appear therefore to have lost the posterior mesentery, instead of only one segment, as in my species.

There is some difficulty in making an exact comparison between the two species, because Perrier's figure (pl. xv. fig. 28) does not agree with his description. In the figure referred to there are but *four* thickened mesenteries, which seem to correspond exactly in their arrangement to the mesenteries of the Australian species. There seems, however, to be a slight difference in position; the last thick mesentery in my species forms the posterior margin of segment 10, if the commencement of the clitellum has been rightly referred by me to the 12th segment. It is, however, not an easy matter to differentiate the two or three anterior segments of the body; and, as Perrier had living specimens at his disposal, it is probable that his enumeration of the segments is more correct than mine. In this case the clitellum in my species begins a segment later than in his.

Integument.—Perrier's memoir contains a detailed account of the structure of the integument (pp. 382–400), illustrated by numerous figures. I cannot, however, altogether reconcile his description and figures, in so far as they refer to the structure of the epidermis, with the appearances presented by my own sections.

In fig. 1 Perrier gives a general view of the epidermis or surface view, in which it is seen to be marked out into polygonal areas, separated by a certain amount of interstitial matter; some of these contain granular bodies (lettered *a* in his figure), while others are without them. Between the setæ are certain very peculiar structures (*g*), which appear in section to be contained in sac-like diverticula (fig. 3) of the chitinous cuticle. The bodies themselves are highly refractive; these evidently correspond to similar structures described by Vejdovsky in *Anachæta.**

In transverse sections through the integument of my specimens of

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* *Monograph. d. Enchytræiden,* p. 21; see also a paper by myself in *Proc. Roy. Soc. Lond.*, 1885, p. 464.

Urochæta, I have met with these peculiar structures in abundance. They stain very deeply in borax carmine, but have the appearance of being formed of some resistant substance, being frequently indented; they lie at the base of the epidermic cells, just in the position in which Perrier has figured them (*loc. cit.*, pl. xii. fig. 2 *g*). There is, however, this difference, that whereas in *U. corethrurus* they almost invariably form a regular line between the several setæ of a segment, being but rarely disposed irregularly, in my species the contrary is the case; they are very frequently irregular in size as well as position, though they form always a continuous row between the setæ, and are not, as far as my experience goes, found elsewhere. Perrier is quite right in stating that the polygonal areas in his figure correspond to cells, but has overlooked the fact (which was not known at the time when he wrote) that the "interstitial" substance is also cellular, and consists of elongated narrow cells, the polygonal spaces being occupied by large glandular cells with granular contents which do not stain. In fig. 2, pl. xii. of Perrier's memoir, a transverse section through the epidermis is figured, which does not at all represent the appearances presented by my sections. In Perrier's figure are represented a series of columnar granular cells, among which are a few peculiar rod-like bodies; these latter I am unable to identify in my preparations, unless, indeed, they correspond to the columnar hypodermic cells. The columnar granular cells appear to be a very inaccurate representation of the large glandular cells, which appear to be much more numerous in *Urochæta* than in *Lumbricus*. Judging by other earthworms, it does not appear to be at all likely that M. Perrier's fig. 2 illustrates a real difference in the structure of the epidermis from my species.

I have frequently noticed, on a superficial view of the epidermic, irregularly shaped refractive bodies, like those figured by Perrier and lettered *a* in his figure (pl. xii. fig. 1), within the glandular cells.

Excretory Organs.—My species of *Urochæta* possesses, like *U. corethrurus*, a pair of large glands in the anterior segments of the body, which have been termed by Perrier "glandes à mucosité." These glands open on to the exterior of the body through a long duct with muscular walls. With regard to

the external orifice, Perrier remarks (*loc. cit.*, p. 460):—"En faisant des coupes dans la région antérieure du corps, nous avous constamment rencontré dans l'épaisseur même des téguments un canal circulaire entouré d'une sorte de sphincter et présentant des cils vibratiles très-reconaissables même sur des individus desséchés. Nous avions d'abord pensé que nous avions sous les yeux la coupe de la portion du canal excréteur des glandes à mucosité qui est logée dans les téguments; mais nous n'avons pu nous convaincre de l'inexactitude [exactitude?] de cette appréciation. Dans nos coupes ce canal s'est toujours montré unique, et les glandes à mucosité ont des orificies excréteurs distincts; de plus, le canal en question nous a paru occuper la partie la plus antérieure du corps; et ces faits sont contraires à la supposition qui nous était d'abord venue à l'esprit." If M. Perrier means to state in the above-quoted words that the excretory duct of the "glande à mucosité" is furnished at its termination with a "sphincter" like that which surrounds the aperture of the nephridia, I am in a position to confirm the correctness of his statement. By a series of transverse sections, I have been able to trace on both sides of the body the duct of this gland to its external opening, and I find that the latter is surrounded by one of these peculiar bodies which Perrier was the first to record in the case of the nephridia. On the other hand, the duct never traversed the body walls except, of course, at the point where it perforates it on its way to the exterior, and the *two* ducts were both perfectly distinct. M. Perrier does not mention whether the single duct which he found in the transverse section was situated laterally or in the median line. I cannot detect any trace of cilia in these canals, which, indeed seem to be hardly needed, as they are physiologically replaced by the muscular walls. The presence of the "sphincter" is evidently an important additional resemblance between the glandes à mucosité and the nephridia.

With regard to the *nephridia*, I am unable to find in my species what Perrier states to be the relations of the internal funnel in *U. corethrurus*. He says (*loc. cit.*, p. 438)—"Les pavillons vibratiles . . . (sont) très-rapprochés de la ligne médiane et appliqués contre la cloison. Il y a là quelque chose de différent de ce qu'on observe chez les naïdiens, où les pavillons vibratiles traversent en général la cloison antérieure de chaque anneau, fait que l'on retrouve aussi chez les *Pontodrilus*. Les Lombrics au contraire semblent d'après les auteurs, se comporter comme les *Urochæta*."

M. Perrier figures (*loc. cit.*, pl. xvi. figs. 38, 39) the isolated nephridia, which obviously could not be detached entire if the funnel were not situated in the same segment as that which bears the external pore. Nevertheless, in my species I observed in numerous cases that the internal funnel of the nephridium is situated in the segment anterior to that which bears the external pore. I was able to prove this point conclusively by a series of longitudinal sections. It may be that Perrier's specimens and mine differ in this respect, which is certainly rather remarkable. Perrier's assertion about *Lumbricus* is evidently a slip. The funnel (figs. 6, 7, 8) of the nephridium recalls that of *Dendrobæna rubida* (Vejdovsky, *System u. Morph. d. Oligochæten*, pl. xiv. figs. 15, 16) in the extraordinary development of cells, doubtfully regarded by Vejdovsky as peritoneal cells, at the apex of the funnel.

A series of remarkable structures, termed by Perrier "glandes posterieures," and described by him as a portion of the excretory system, now remains for consideration.

These bodies are found as in *U. corethrurus* in the hinder region of the body, but appear to be more numerous than in that species, which has about forty pairs occupying as many segments.

M. Perrier gives a figure of one of these glands (*loc. cit.*, p. xvii. fig. 47), which only partially indicates their structure, as seen in my own preparations. They are somewhat pear-shaped, and terminate in a long slender peduncle, which disappears among the coils of the nephridial tubules. Perrier supposes that they open in common with the latter on to the exterior, but was unable to detect the orifice. Mr Benham* has detected these peculiar glands—"pyriform bodies"—in his genus *Urobenus*, and his description of their minute structure agrees pretty closely with my own observations; these glands open in *Urobenus* ventrally of the lower pair of setæ, while the nephridia open by the dorsal setæ.

Fig. 4 represents one of those glands in *Urochæta* in longitudinal section, reconstructed from a series of sections. It will be seen that its structure is closely similar to that of the same glands in *Urobenus*. The lumen of the gland is lined by a single row of peculiar cells, rounded and of large size, and each furnished with a distinct nucleus. These cells are evidently larger in proportion, and not so columnar as the corresponding cells in the pyriform vesicle of *Urobenus;* the rest of the gland lying to the outside of these cells is occupied (fig. 5) by a granular substance, with minute darkly staining bodies scattered throughout it (nuclei ?). The lumen ceases some little way in front of the apex of the gland, which is here entirely made up of the granular nucleated substance. It is permeated by blood capillaries derived from the vessels which supply the nephridia. The pear-shaped glandular region of the pyriform vesicle has the structure just described; distally it communicates with a slender muscular duct, which passes gradually into the substance of the gland. The latter is bent upon itself, as indicated in Perrier's figure, so that the duct runs parallel with the gland. But while in *U. corethrurus* and in *Urobenus* the duct is directed towards the nerve cord, the flexure in my *Urochæta* is exactly in the opposite direction. The rounded cells lining the lumen gradually decrease in importance, and the granular substance, with its interspersed nuclei lying to the outside of these cells, eventually disappears; coincidently with these charges the duct of the gland acquires a delicate muscular coat, and the lining epithelium finally becomes a flattened layer of cells. I have traced this muscular sac to its opening on to the exterior in common with the nephridium. Fig. 4 shows the termination of the duct in the rosette-like organ which here as elsewhere guards the orifice of the nephridium. The pyriform vesicle, therefore, is *anatomically* a diverticulum of the nephridial duct in this species.

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* *Quart. Jour. Mic. Sci.*, 1886, p. 87, pl. viii. figs. 10, 21.

Spermathecæ.—These organs are present to the number of three pairs; they are situated in segments 7, 8, and 9, and the aperture is in each case placed quite close to the anterior margin of the segment. The spermathecæ of this species are excessively delicate organs, and are often for this reason difficult to distinguish; they are also of very small size, as compared with the spermatheca of many other worms. The smallness of size is manifest rather in their breadth than in their length; when stretched out they reach rather further than across the segment which contains them. These organs are somewhat club-shaped; the distal region is extremely narrow, but widens out gradually passing backwards, and finally becomes dilated into an oval sac. The spermathecæ sometimes lie straight, and are sometimes coiled into a circle. The walls of the spermathecæ are very thin, owing to the slight development of muscles and the character of the lining epithelium, consisting as it does of flattened cells; these structural features, together with the superficial covering of rounded, vesicular, peritoneal cells, and the general shape of the organs, gives the spermathecæ a very strong resemblance to the diverticula of the nephridia figured by myself in *Acanthodrilus novæ-zelandiæ.*[*] In view of a possible homology between the spermathecæ and such diverticula, it is worth while to record the points of similarity between the two series of organs. Furthermore, I may remark that in a large number of individuals, all fully mature, there was no increased development visible in the spermathecæ, which undoubtedly have a certain appearance of immaturity.

The general shape of the spermathecæ is very like that of the spermathecæ of *Diachæta,*[†] but they appear to be considerably smaller in the present species, and also differ in that their apertures on to the exterior are at the anterior, instead of at the posterior, boundary of their respective segments.

In *Urochæta corethrurus*[‡] there are also three pairs of spermathecæ not unlike those at present under discussion in shape, and opening like them at the anterior margin of their segment; they are situated, however, rather further back (in segments 8, 9, 10); further, in both *Urochæta* and *Diachæta* the spermathecal segments contain nephridia.

Perichæta newcombei,[§] n. sp.

This species is represented by eight individuals, of which four are sexually mature, with a fully developed clitellum.

The colour of the species is a dark purple upon the dorsal surface, gradually passing into a yellowish-brown upon the ventral surface; the intersegmental furrows dorsally, as well as ventrally, are of the

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[*] *Proc. Zool. Soc.*, 1885, pl. lii. fig. 5.

[†] Benham, *loc. cit.*, pl. ix. fig. 29.

[‡] Perrier, *Arch. d. Zool. Exp.*, t. iii. (1874), p. 518, pl. xiii. fig. 12 *pc;* pl. xvii. fig. 49.

[§] Named after Mr S. Prout Newcombe.

same colour as the ventral surface; the clitellum also is distinguishable on the dorsal surface by its yellowish tinge.

It is interesting to note that the colour of this species is exactly that of a species of *Perionyx*, from the Philippine Islands, the characters of which I have recently described in a paper communicated to the Zoological Society of London.*

The *preoral* lobe does not divide the circumoral segment. *Dorsal pores* are present between all the posterior segments of the body; in the four mature individuals the first pore is situated between the 5th and 6th segments; the clitellum is marked anteriorly and posteriorly by a conspicuous pore.

The *setæ*, as in other species of the genus, are disposed in a continuous series, occupying the middle line of each segment; they are present on the clitellar segments.

The *clitellum* occupies three segments, Nos. 14, 15, 16; as in all species of *Perichæta* it is developed round the whole circumference of the body.

The *male* and *female generative pores* are placed in exactly the same situation as in other species. The female pore is placed upon the 14th segment, within the row of setæ in the middle line; the male pores are upon the 18th segment, at some little distance from each other, also within the row of setæ.

The *apertures of the spermathecæ* are between 7–8 and 8–9.

A very striking external character of this worm is caused by the great development of *genital papillæ.*

These are developed on the preclitellar segments (fig. 10), as well as on the segments which immediately precede, and on those which follow the 18th segment.

The arrangement of the preclitellar papillæ presents some individual variation, which is probably due to the fact that some of the specimens are more fully mature than others.

In one example the papillæ were more numerous than in any of the others. The 13th segment is furnished with a single papilla in the ventral median line; the 11th and 12th segment have each three papillæ close together, one being median, and the other two disposed symmetrically, one on either side; the 10th segment has four papillæ, of which the middle ones correspond in position to

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* *Proc. Zool. Soc.*, 1886, p. 298.

the median papillæ of the two succeeding segments; the 9th segment has a single papilla, corresponding in position to the outermost right-hand one of the 10th segment, the others being indistinct; the 7th and 8th segments have each a single median papilla. In another example the 12th and 13th segments have a single median papilla; the 10th and 11th segments have each three papillæ; the 7th, 8th, and 9th a single median papilla.

Two other examples present an arrangement of the genital papillæ nearly identical with that last described, the only difference being that the papillæ on segments 7 and 8 are wanting.

In every case the papillæ present the appearance of a circular disc similar in colour to the clitellum, and surrounded by a whitish line; the greater part of the disc is placed in front of the row of setæ.

The postclitellar papillæ are not so distinct as the preclitellar. The whole of the ventral integument on the 17th, 18th, and 19th segments lying between the male aperture is whiter in colour than the rest, which renders it very difficult to map out the position of the papillæ. The 17th segment appeared to have a row of these papillæ; in the 18th and 19th segments I could only distinguish two pairs of papillæ, one placed outside of the male pore on the 18th, and in a corresponding position on the 19th segment, and the other placed below, and both inside of the male pore. The 20th segment has a median row of papillæ (3 or 4), the 21st segment has three median papillæ. The postclitellar papillæ are considerably smaller than the preclitellar. I am inclined to think that the whitish appearance of the integument between the male generative pores is due to the crowding together of a row of papillæ, which become distinct and separate on the 20th, and especially on the 21st segment.

The large *pharynx* extends back to about segment 3; the *gizzard* occupies segments 4, 5, and 6; it is important to notice that in every case the segments in which the gizzard lies are separated from each other by distinct, though rather delicate, mesenteries; this fact is worth recording, because in many species of *Perichæta* (and other genera) the gizzard occupies two segments, and the median mesentery has disappeared; there seems to be, however, some connection between this condition and the position of the gizzard.

In the present species the gizzard lies anteriorly to the spermathecæ; in those species where a mesentery has disappeared the gizzard lies further back, and in the same segments with some or all of the spermathecæ.

Calciferous glands are present in segments 10, 11, 12; they are, however, rather dilatations of the lumen of the œsophagus than distinct and separate glands.

The *testes* are situated in the 10th and 11th segments, close to the nerve cord and on either side of it. Dr Bergh is perfectly right in his statement * that the testes and vesiculæ seminales of *Perichæta* are in all essentials similar to those of *Lumbricus.* The testes in the present species are small digitate glands, and are enclosed by the vesiculæ, as is also the nerve cord. The vas deferens passes along the body just below the testis; the funnels of the vasa deferentia open into the vesiculæ seminales, which organs extend from the 9th to the 12th segment.

The *ovaries* are very large, and are situated in the 13th segment.

The *prostates* occupy the usual position.

The *spermathecæ* are present to the number of two pairs, situated in segments 8 and 9; the large somewhat pear-shaped pouch is provided with a small diverticulum on the dorsal side.

The only species of *Perichæta* recorded from Australia are two species, *P. australis* and *P. coxii*, described recently by Mr Fletcher.† It is evident that my species agrees with these two in a great many points; in the first place, there appear to be no intestinal cæca; secondly, the shape and location of the spermathecæ appears to be identical in all three species. The first point of agreement is, however, of more importance than the latter. In a good number of species of *Perichæta* there are two pairs of spermathecæ situated in segments 8 and 9, and each furnished with a slender cylindrical diverticulum; it will be interesting to know if the absence of intestinal cæca is characteristic of other Australian species of the genus.

The present species, however, differs from both its Australian congeners in the presence of vesiculæ seminales in all of the segments from 9–12 inclusive. Fletcher states that these structures are

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* *Zeitschr. f. wiss. Zool.*, 1886.

† *Proc. Linn. Soc. N.S. W.*, June 1886, p. 561.

absent in segments 10 and 11 in his species; if this difference is not really due to difference of age, it is clearly of great importance as a distinctive character.

The arrangement of the nephridia is apparently very like what has been described in *P. australis* and *P. coxii*, particularly in the latter species. Fletcher's description is as follows:—"The segmental organs consist of tufted glandular masses, which are large, stalked, and dendriform in some of the most anterior segments, but smaller and inconspicuous elsewhere." I found these structures very conspicuous indeed, and in the 14th and a few succeeding segments they have a very strong superficial resemblance to the ovaries, with which organs their position almost exactly corresponds.

The most characteristic point of difference between my species and the other two is the number and position of the genital papillæ; a comparison of my description with that given by Mr Fletcher of *P. australis* and *P. coxii* will show that the species differ greatly in this respect. Mr Fletcher like myself appears to have examined a considerable number of specimens.

Perichæta upoluensis, n. sp.

This species of *Perichæta*, like the last, is mainly characterised by the number and arrangement of the genital papillæ. It is a native of the island of Upolu, in the South Pacific; I am indebted to Mr R. Damon, of Weymouth, for the opportunity of examining three specimens.

It is an average-sized species, measuring 5 or 6 inches in length.

The apertures of the spermathecæ are between 7–8 and 8–9.

The single aperture of the oviduct is upon segment 14.

The pores of the vasa deferentia are upon segment 18. Each pore is surrounded by a circular area of integument which is marked off from the rest.

The *clitellum* consists of only two segments, Nos. 14 and 15.

The genital papillæ are very small, compared, for example, with those of the last species; they occur in the neighbourhood of the spermathecæ as well as of the male generative apertures.

There is a single papillæ on segment 9, situated in the median ventral line and anteriorly to the row of setæ. The rest of the genital papillæ (so far as my specimens enable me to speak positively) are situated after the clitellum, *i.e.*, in the neighbourhood of the male generative openings. Each of the segments 16–20 (inclusive) is furnished with a single median papilla, which occupies a precisely similar position to that occupied by the median papilla of segment 9, that is to say, it lies near to the anterior border of the segment (see fig. 11). The 18th segment possesses, in addition, a pair of papillæ, situated just within and close to the male generative orifices; these papillæ are almost on the border line between this and the following segment. The next segment (No. 19) has also an additional pair of genital papillæ; these are placed below and a little to the outside of the generative pores; hence they are placed very close to the anterior border of their segment.

In its internal structure this species does not present any remarkable features.

The *gizzard* is in segments 8 and 9, and as usual these segments are not separated by a mesentery.

In the same two segments are situated the *spermathecæ* (fig. 12), which (see p. 173) are not very different in shape to those of the last species.

The *vesiculæ seminales* are in segments 11 and 12.

The *ovaries* are in segment 13.

The termination of the vas deferens is furnished with a prostate gland, which has the usual lobulated structure.

The hearts are in segments 12 and 13, as is generally the case in *Perichæta.*

Perichæta antarctica, Baird.

Megascolex (*Perichæta*) *antarctica,* Baird, *Proc. Linn. Soc.*, vol. xi. (1873) p. 96.

This species has been described by Baird from a specimen in the British Museum in the following terms:—"Body consisting of about 180 rings. Setæ, surrounding the body, short, black, rather distant. Rings not keeled; larger and more distinct at the anterior extremity, closer at the posterior end, and all smooth. Length 7 inches." Capt. F. W. Hutton, in his "Catalogue of the hitherto described Worms of New Zealand,"* mentions this species, which is a native of New Zealand, and simply quotes Baird's description.

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* *Trans. New Zealand Instit.*, vol. xi. (1878) p. 317.

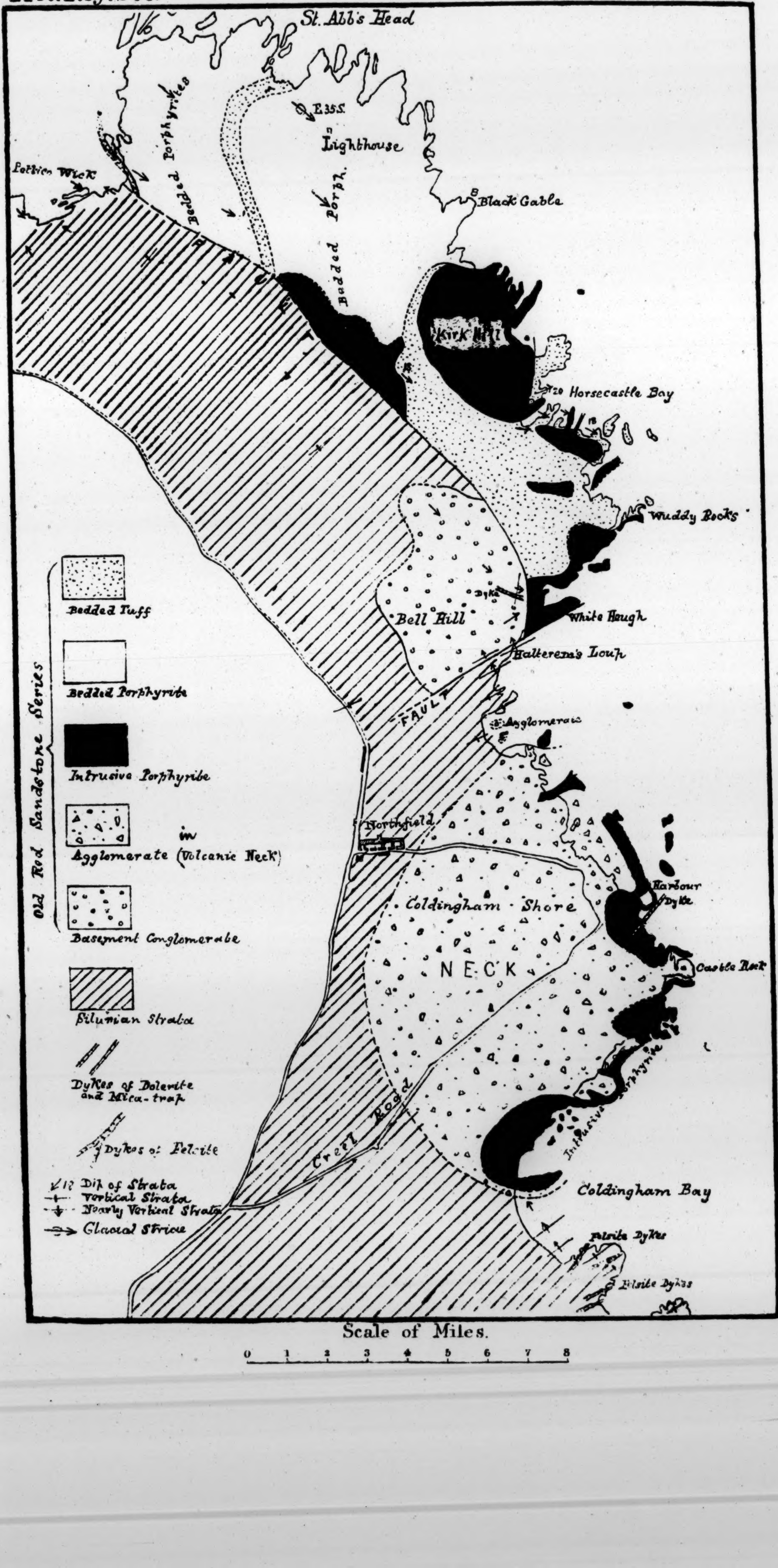
It is perfectly clear that the above-quoted specific diagnosis is entirely insufficient to discriminate the species from many other *Perichætæ;* but an examination of the specimen itself leads me to believe that it is a distinct species. I am unable to give any anatomical description, but the worm exhibits an external character, overlooked by Baird, which is of some value as an aid to discriminating the species. The male genital pores are as usual situated upon the 18th segment of the body, and at some distance from each other; the 17th and 19th segments are each furnished with a single median genital papilla placed exactly in the centre of the segment, and therefore interrupting the line of setæ. The number and arrangement of the genital papillæ seem to be, so far as our knowledge goes, good characters for discriminating the different species of *Perichæta;* although the number is apt to vary somewhat (see p. 171) at different stages of maturity; the number of papillæ in the present species would have to be very largely increased to come up to the number which are characteristic of *Perichæta newcombei* (p. 171), the only other species of *Perichæta* which has genital papillæ in the *median ventral line* on the 17th and 19th segments.

EXPLANATION OF PLATE V.

Fig. 1. *Acanthodrilus dissimilis.*
Fig. 2. *Acanthodrilus neglectus.*
Fig. 3. *Neodrilus monocystis*, section through prostate; *m*, muscular duct; *gl*, glandular region.
Figs. 4–9. *Urochæta*, sp.
Fig. 4. Median longitudinal section through glandular appendix of nephridium; *d*, glandular *cul-de-sac*; *c*, epithelial lining; *b*, muscular region; *a*, "sphincter" surrounding aperture; *m*, mesentery.
Fig. 5. Transverse section through glandular appendix and a portion of nephridium; *n*, nephridial tubule; *c*, *d*, regions similarly lettered in fig. 4.
Figs. 6, 7, 8. Sections in various planes through nephridial funnel; *p*, peritoneal cells; *p g*, peculiar agglomeration of peritoneal cells in the funnel.
Fig. 9. Transverse section of a nephridial tubule from hinder end of body; *c*, peritoneal cells; *b*, blood corpuscles; *n*, nephridial tubule.
Fig. 10. *Perichæta newcombei*, ventral aspect.
Fig. 11. *Perichæta upoluensis*, ventral aspect.
Fig. 12. Spermatheca of last species.

Geological Map of St. Abbs Head, etc,
Based on that of the Geological Survey.

Proc. Roy. Soc. Vol XIV. Pl. VI.



6. Geology and Petrology of St Abb's Head. By Professor J. Geikie. (Plate VI.)

I. INTRODUCTION.

The observations recorded in this paper have reference chiefly to the coast-sections at St Abb's Head and Coldingham Shore. The district was geologically surveyed some twenty-five years ago by my brother, Dr A. Geikie, and subsequently described by him in the Memoirs of the Geological Survey.* Since the publication of that memoir, no further examination of the ground in question appears to have been made. During the past summer I visited the neighbourhood, principally for the purpose of studying the igneous rocks which are so well exposed in sea-coast sections. At the date of the Government Survey of Eastern Berwickshire the aid of the microscope had not yet been invoked by field-geologists for the purpose of determining rock-species, and I was therefore curious to compare the igneous rocks of that region with those of similar age which I had studied elsewhere in Scotland, and more especially with the bedded and intrusive porphyrites and tuffs of the Cheviot Hills and the Sidlaws.

The rocks of the district under review belong, as my brother has shown, to two great systems—the Silurian and the Old Red Sandstone. From Pettico Wick Harbour in the north, to Coldingham Bay in the south, the coast cliffs are composed almost exclusively of rocks of Old Red Sandstone age—Silurian strata appearing only for a short interval, a little to the north of Coldingham Shore. The latter reappear on the south side of Coldingham Bay, and continue along the shore to Callercove Point, in the neighbourhood of Eyemouth. Inland from St Abb's Head and Coldingham Shore they extend for many miles. (See Map, Plate VI.)

II. THE SILURIAN ROCKS.

To the description of the Silurian strata given in the Geological Survey's Memoir, I have very little to add. They consist chiefly of greywackės and shales, generally inclined at high angles, and arranged in a series of more or less sharp anticlinal and synclinal folds, which have an average N.E. and S.W. trend. In their least altered condition, they are well seen in the magnificent cliffs that extend westward from Pettico Wick Harbour. Between Coldingham Bay and Callercove Point they are often much crushed, crumpled, twisted, shattered, shifted, and confused—the irregular puckerings and convolutions forming an interesting study. They are minutely cracked and fissured in all directions, the fine cracks and fissures being most frequently filled with white quartz, or with hæmatite and limonite. Where the strata are most highly contorted, they frequently become seamed with a close, irregular network of small veins and mere threads of quartz, the meshes between which are often less than the 16th part of an inch in diameter.

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* Geological Survey Memoirs: *The Geology of East Berwickshire.*

Intrusive Felsite in the Silurian.—Through these excessively contorted rocks ramify here and there tortuous dykes and veins of felsite. The junction between those dykes and the rocks traversed by them is generally well marked. But here and there it is much confused—the fine-grained greywackés being baked and altered, and having the same pale-grey colour as the felsite, so that the line of parting between the two can hardly be distinguished by the unassisted eye. Under the microscope, however, the crystalline and fragmental rocks are readily discriminated. These felsites are confined to the Silurian areas. Nowhere, so far as I saw (and I traversed a considerable area round Coldingham and Eyemouth), do any of the felsitic intrusions penetrate rocks of Old Red Sandstone age.

The rock of these dykes is grey or pale pinkish-grey in colour, compact, sparingly porphyritic, with microscopic crystals of quartz and felspar,—having, in short, the appearance of typical felsite. Here and there veins of white quartz seam the dykes. Viewed under the microscope the rock exhibits a microfelsitic base, scattered through which are abundant small crystals of orthoclase, and a few larger ones of the same mineral. Oligoclase also appears in well-developed crystals, and to the same species some of the smaller crystals of felspar seem to be referable. Both felspars show alteration into saussurite, but this is most frequently the case with the orthoclase. Crystals of quartz, often much corroded, but not unfrequently showing well-defined pyramidal forms, are common; and they generally contain fluid cavities, sometimes in very great abundance. One or two thin spicules of a dichroic mineral, probably mica, were seen, but only in one section. The most remarkable feature presented by these felsites is the appearance of minute veins which traverse the felsite irregularly, not infrequently crossing and anastomosing with each other at all angles. They vary in width from a mere line up to $\frac{1}{10}$ or $\frac{1}{5}$ of an inch in diameter, and consist chiefly of quartz and felspar, apparently orthoclase. The quartz undoubtedly predominates, but here and there felspar, or saussurite which replaces the felspar, forms the chief ingredient. The quartz occurs in irregular crystalline aggregates and granules, often crowded with fluid cavities, and frequently containing enclosures of saussurite, and occasionally epidote. The felspar also forms irregular crystalline aggregates, but is most usually replaced by saussurite. Not infrequently it forms intergrowths with the quartz, so as to give the veins a micropegmatitic structure. Sometimes the walls of the veins are smooth and even; at other times the quartz and felspar (saussurite) seem to indent the walls, as in the so-called "segregation veins" of granite. The veins now described appear to be confined, as a rule, to the felsite, but occasionally they pass outwards from the latter into the adjacent greywacké.

III. The Old Red Sandstone Series.

The rocks assigned to the Old Red Sandstone are principally of igneous or aqueo-igneous origin. There is one small patch of conglomerate, however, which forms the upper portion of Bell Hill, near the village of Coldingham Shore. This is indicated on the Geological Survey's map as of Upper Old Red Sandstone age. I think it really belongs to the lower division of the series; at all events it is older than the bedded igneous rocks to be described presently. It rests directly upon the Lower Silurian, and is in fact composed exclusively of rounded fragments of greywacké, &c., derived from that formation. It nowhere overlies the igneous rocks referred to, nor does it contain any admixture of fragments of these. Its junction with them is, in short, a dislocation or fault, which has a downthrow to the N.E. (see fig. 1).

The bold headland of St Abb's is composed entirely of crystalline and fragmental igneous rocks, some of these being bedded and contemporaneous, and others amorphous and intrusive, while the igneous rocks at Coldingham Shore and Coldingham Bay appear to be exclusively intrusive in character. I shall describe the rocks of those two areas separately, although they probably all belong to the same period of volcanic activity.

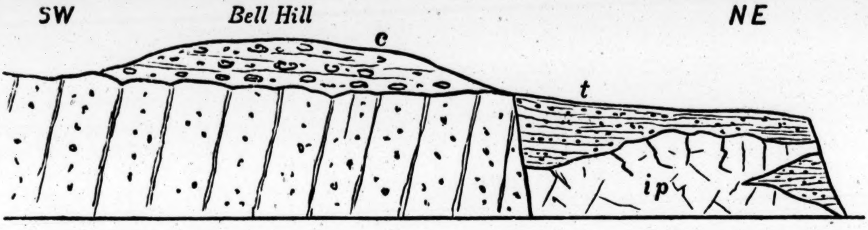

FIG. 1.—Section across Bell Hill, showing relation of Basement Conglomerate (*c*) to Igneous Rocks of St Abb's Head (*t*, *ip*).

(*a*) *Rocks of St Abb's Head.*

The headland of St Abb's Head extends from Pettico Wick south-east to the White Heugh—a picturesque cliff and favourite resort of sea-birds—about ¼ of a mile north-west of Coldingham Shore. The headland presents to the sea a bulwark of wild rugged precipices, indented with numerous little bays and coves, only a few of which are accessible from the land. It is separated from the rolling Silurian uplands behind by a well-marked hollow that extends south-east from Pettico Wick in the direction of the White Heugh. The headland, as thus defined, is described by the Geological Survey as consisting in the south-east partly of fragmental igneous rocks, and partly of intrusive "felstone"; while between Horsecastle Bay and Pettico Wick the area is coloured as intrusive felstone alone. Various sections laid bare since the time the ground was examined by the Geological Survey show that the northern part of the headland is made up chiefly of bedded porphyrite with some intercalated layers of tuff. Of the latter the only good exposures seen are at Pettico Wick Harbour and on the side of the road leading thence to the lighthouse. The dip of these rocks is clearly towards the south-east, the whole forming an ascending series, from Pettico Wick to the Wuddy Rocks, with an approximate thickness of 1200 feet (see fig. 2). Towards the south-east the beds are invaded by larger and smaller masses and dykes of intrusive crystalline rock. As the dip of the igneous series does not exceed 18° on an average, it is obvious that the junction between these rocks and the vertical Silurian strata can only be a fault, having its downthrow to the north-east. This is the dislocation already referred to as bringing down the bedded igneous rocks of the Old Red Sandstone series against the basement conglomerate of Bell Hill. It is well seen at Rutherford Brae. Another fault, running at right angles to that just described, forms the boundary between the Old Red and the Silurian on the south side of Bell Hill. It is seen in section in the sea-cliffs at Halterem's Loup, where the Old Red Conglomerate is turned up at a high angle against the Silurian greywackés and shales (see fig. 3).

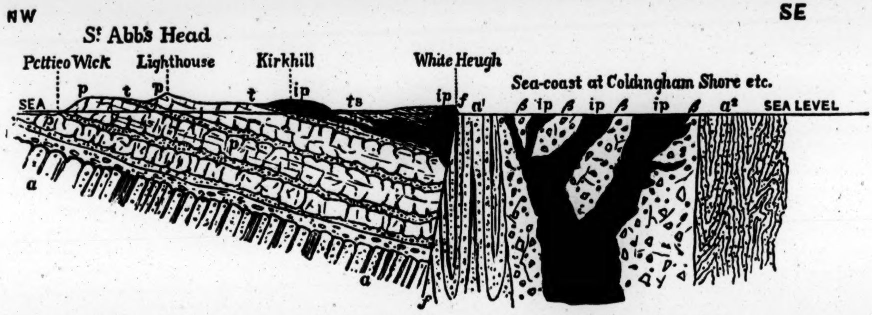

FIG. 2.—General Section across St Abb's Head to Shore of Coldingham Bay (scale 3 in. to 1 mile ; horizontal and vertical scale the same).

a, a, Probable position of Silurian strata under rocks of St Abb's Head. *b,* Conjectural position of basement conglomerate of Old Red Sandstone series. *c,* Inferred base of Volcanic series. *p, p, p,* Bedded porphyrites = andesitic lavas. *t, t, t,* Bedded porphyrite-tuff; at *ts* largely composed of small scoriæ. *β, β, β,* Agglomerate and tuff in volcanic neck. *ip, ip, ip,* Intrusive porphyrite. *f, f,* Fault at Halterem's Loup. a^1, Silurian strata somewhat altered ; a^2, Silurian strata much contorted and altered.

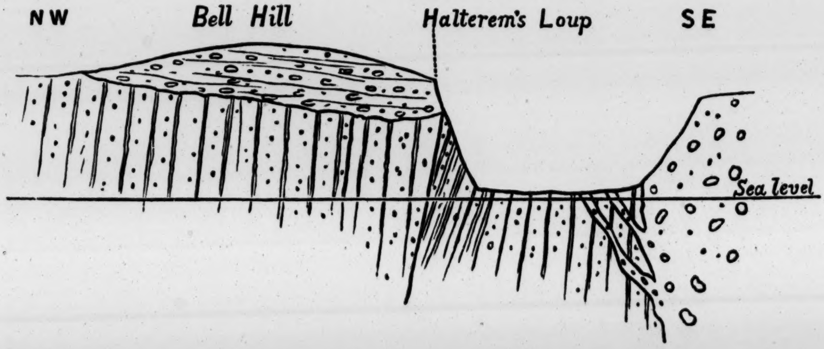

FIG. 3.—Section across Bell Hill, showing relation of Basement Conglomerate to the Silurian strata.

(1) *The Basement Conglomerate.*—The conglomerate of Bell Hill would thus appear to be the oldest member of the Old Red Sandstone series. It consists principally of water-worn shingle and gravel, set in a matrix of arenaceous and argillaceous matter. The stones, as mentioned already, have all been derived from the contiguous Silurian strata. The lower appear to be upon the whole coarser than the upper beds, stones 6 inches and more in diameter being common in the former, while the latter are rather conglomeratic and pebbly grit and sandstone than conglomerate. The beds dip towards the south-east, but are *turned up* against the N.E. and S.W. fault, while they *trend steeply down* towards the N.W. and S.E. fault. The series is probably about 100 feet in thickness.

(2) *The Igneous Series.*—The lowest beds seen on the north side of the N.W. and S.E. fault is a bedded porphyrite which is overlaid by a thick layer of agglomeratic tuff. Above this comes a succession of bedded porphyrites, about 250 feet or so in thickness, and these beds are succeeded by 40 to 50 feet of various tuffs, which dip in their turn under a second group of porphyrites. These last do not appear to exceed 250 or 300 feet in thickness, and are overlaid by some 400 feet of bedded tuffs.

Bedded Porphyrites.—These rocks having all the same character, one general description will suffice. They are for the most part fine-grained, purplish-blue or greyish-blue in colour, but frequently stained brown or red with much diffused ferric and hydrous ferric oxide. The joint-faces especially are often coated with hæmatite and limonite, while thin veins and threads of the same minerals are common. The rocks do not differ in general appearance from the porphyrites of Old Red Sandstone and Lower Carboniferous age which occur elsewhere in Scotland. They are, upon the whole, not so markedly porphyritic with plagioclase as the porphyrites of other districts, but closely resemble such fine-grained rocks as that of Blackford Hill, and similar rocks met with in the Braids and Pentlands. They are often highly scoriaceous and amygdaloidal above and below, and not infrequently contain, both in their upper and under portions, irregular areas of fine-grained tuff, consisting of amorphous, dust-like material, and comminuted débris and small lapilli of highly porous porphyrite (see fig. 4). In the upper parts of some of the old lava-flows this tuff appears to fill up irregular clefts and vein-like cavites in the porphyrite, while in other places it appears involved in the porphyrite in such a way as to suggest that it may probably consist of portions of the shattered scoriaceous crust of the porphyrite, broken up and incorporated in the underlying mass while that was still in a fluid or pasty condition. Occasionally so much of this tuff-like matter is enclosed in the porphyrite that one is sometimes in doubt as to whether the whole rock is not fragmental. Microscopic examination, however, clearly shows that this is not the case—the angular and sub-angular lapilli and cinder-like fragments being completely surrounded by or embedded in finely crystalline rock. The porphyrites are all more or less weathered and earthy to some depth, and it is thus difficult to obtain very fresh fractures. They form a series of low broken escarpments facing the north-west, each escarpment marking the outcrop of a bed. The beds appear to be of variable thickness, some measuring about 15 feet, while others may reach as much as 50 feet or more.

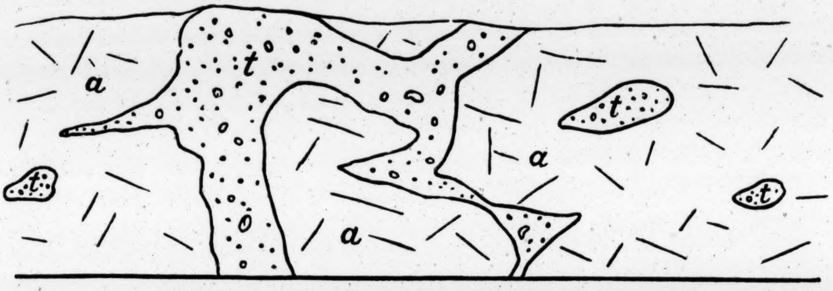

FIG. 4.—Red Tuff (*t, t, t*), enclosed in Porphyrite (*a, a, a*). Enclosures vary from an inch or less up to a foot or more in diameter.

Examined under the microscope, these rocks show a ground-mass of colourless microliths and minute lath-like crystals of plagioclase, diffused through which there is usually more or less non-differentiated red ferritic matter. In some cases the ground-mass seems to be composed chiefly of this unindividualised matter, with microliths and small rods of plagioclase scattered less abundantly through it. This is more especially the case with the amygdaloidal parts of the rock, where occasionally the ground-mass consists almost exclusively of non-differentiated matter, only a few recognisable microliths making their appearance. There can be little doubt that this unindividualised substance is simply the result of devitrification, and that originally these rocks contained no inconsiderable proportion of glassy matter, especially in their upper and under portions. In a number of the sections examined, however, no trace of devitrified matter was observed, the ground-mass in such cases consisting of an aggregate of microliths and minute crystals of plagioclase, closely felted together, but containing interstitially abundant granules of ferrite, or magnetite passing into ferrite, along with minute granules of pale greenish-yellow, and yellow serpentinous matter and limonite, which are doubtless alteration products replacing hornblende or pyroxene, or both. The porphyritic ingredients of these rocks are seldom prominent. Small and large lath-like crystals of plagioclase are not uncommon, the larger crystals seldom exceeding 1 mm. in length, and they are mostly broken. They sometimes show fine zonal structure. The larger crystals seem to be most common when the ground-mass is composed chiefly of devitrified matter, with very minute microliths. Pseudomorphs after hornblende and augite appear more or less plentifully. These consist partly of yellow or greenish-yellow serpentine, sometimes veined with chrysolite, and partly of limonite. Not infrequently the pseudomorph is composed internally of serpentine, the outlines of the crystalline form being defined by limonite, or magnetite and limonite. Very often the limonite forms a meshwork of veins running through the serpentine, and occasionally these veins approximate in direction to the cleavage-planes of the original mineral. In many cases the shape of the pseudomorph gives one no hint as to whether the replaced mineral was augite or hornblende. Very often, however, the form is that of hornblende. This is specially the case with such pseudomorphs as have well-marked ferritic borders. In these one sees that the original mineral must have been broken, and more or less corroded, the ground-mass having often eaten into the heart of the crystal. A few pseudomorphs show very distinctly the form of augite, and some of these also contain inclusions of the ground-mass. Many of the pseudomorphs are quite amorphous, some composed almost entirely of serpentine, others almost exclusively of limonite: what proportion of these may represent hornblende, and what proportion augite, it is impossible to say. Of pseudomorphs having more or less definite forms, those after hornblende appear to be the most numerous, and this is probably the case with the amorphous ones also. Fluidal structure is occasionally marked, the microliths and lath-like crystals of plagioclase being grouped round the larger porphyritic ingredients. In some of the rocks minute amygdaloidal cavities abound, and this even at a distance from the upper and under surfaces of the flows. The cavities are filled generally with calcite, or with calcite and quartz, or chalcedony. More rarely zeolites are present. The larger amygdules in the more scoriaceous portions of the rocks consist of the same minerals, calcite predominating. Most of the rocks are more or less deeply stained with red ferritic matter.

The tuffaceous areas in the porphyrites afford an interesting study. Occasionally they consist of broken or crushed scoriæ of one and the same kind of rock completely embedded in porphyrite. These small lapilli or scoriæ are generally finely and abundantly porous, and show a dark devitrified ground-mass in which minute microliths are more or less plentiful. They might quite well represent fragments of the more scoriacous and glassy portions of the same rock as that in which they are enclosed. In some places the tuffaceous matter consists of finely comminuted débris of the same or some closely similar rock, together with finer grit, and rounded pseudomorphs of serpentine and limonite after hornblende or pyroxene. The larger scoriæ and lapilli rarely exceed a hazel-nut in size. They are enclosed in the porphyrite in such a way as to show that this rock was in a fluid or pasty condition at the time they became embedded, for the crystalline and microlithic ground-mass lies between and among them. The tuffaceous areas now described appear to be confined to the upper and lower parts of the lava-flows, but they occasionally occur nearer the middle. They are generally distinguished from the rock in which they are enclosed by their deeper red colour, a character which they have in common with the beautiful red tuffs of Horsecastle Bay.

Bedded Tuffs.—These rocks present themselves amongst the porphyrites at three horizons, but it is quite possible that thin layers of similar fragmental materials, concealed by turf and superficial débris, may occur at other levels. A continuous section across the outcrops of the igneous series would probably show that each bed of porphyrite was underlaid by tuff or tuffaceous deposits. The lowest bed of tuff is seen at Pettico Wick Harbour. This is a coarse tuff or agglomerate of fragments of porphyrites, which are generally dark purplish blue and red in colour, and more or less highly amygdaloidal. The stones are angular and subangular in form, and show no regular arrangement. They vary in size from small lapilli up to blocks of more than 1 foot across—some measuring 2 feet in diameter. Examined under the microscope, these porphyrites show the same structure and composition as those already described. The tuff rests upon a very irregular surface of porphyrite, and is overlaid, in a like irregular manner, by a bedded porphyrite, which is very vesicular and amygdaloidal at the line of junction. The junction is somewhat confused in places by minor slips and faults.

On the side of the road leading from Pettico Wick to the lighthouse various porphyrites are exposed,—some of which show the curious vein-like and irregular inclusions of fine tuff already described. These beds are overlaid by a considerable thickness of well-bedded shaley tuffs, generally red in colour—the tuff being fine-grained, and composed of comminuted débris of porphyrites. Some of the beds contain many lapilli of larger size scattered through their mass. They rest upon a dark purplish-blue amygdaloidal porphyrite, and are traversed intrusively by a thin sheet of fine-grained porphyrite. The junction is slightly confused, as at Pettico Wick, by faulting.

But by far the most extensive succession of tuffaceous strata is that which occurs at the south-east end of the headland of St Abb's. These rocks are generally well bedded, and have a prevalent red colour. They vary in texture from tuffaceous mudstones, in which only grit and small lapilli occur, up to coarse-grained tuffs, in which the included fragments may reach 1 foot or more in diameter. The most common rock is a tuff composed of comminuted débris and small lapilli, often not larger than a hazel-nut in size, but sometimes measuring 2 or 3 inches across. The grit and lapilli are of all shades of red, purple, yellow, and blue—the red strongly predominating—so that the resulting tuff is finely mottled. In many cases decomposition products, particularly calcite, permeate the rock in all directions—giving rise to irregular white splatches, veinings, and tangled thread-like areas—which show well on the warm red ground of the tuff. Other parts of the tuff might be described as a breccia of subangular and angular fragments, chiefly of amygdaloidal porphyrites—the stones being set in a meagre matrix of comminuted débris. These rocks dip S.E. at 18° to 20°. They are abundantly pierced and traversed by dykes and irregular sheets and masses of porphyrite, which will be described later on. In some places, especially in the area extending from Raven's Brae to Horse Castle, the tuffs have been considerably baked and altered, so that it is sometimes difficult in hand-specimens to distinguish between what is tuff and what intrusive porphyrite. The fragmental character of the altered tuff, however, is quite apparent under the microscope.

The fragments in these tuffs appear to consist exclusively of varieties of porphyrite; at all events I could find no other rock. Most of the lapilli are vesicular and amygdaloidal—very many are highly so, and have all the appearance of scoriæ. The amygdules being usually white are generally very conspicuous. A microscopic examination of many of these included fragments shows that they consist exclusively of porphyrite—the ground-mass being generally vitreous, stony, or devitrified, but occasionally microlithic. The description given above of the small lapilli, which occur in the tuffaceous areas of the bedded porphyrites, holds equally true of the lapilli of these bedded tuffs. A great number of the small stones are evidently fragments of a vitreous scoriaceous porphyrite, and from their highly vesicular character they might well have floated in water at the time of their ejection—they are, in short, mere cinders. The fragments which show a microlithic ground-mass often contain also pseudomorphs after hornblende or pyroxene, and are evidently true porphyrites. Red ferritic matter saturates most of the stones; but not a few of these are dark grey or blue, and it is such fragments which show best the original vitreous character of the ground-mass. Here and there amongst the fine comminuted matter, or dust and grit, of the tuffs, small patches of serpentine and limonite occur, and these, in some cases at least, almost certainly represent hornblende or pyroxene.

The tuffs are thus composed essentially of volcanic detritus. In none of the specimens examined by me did I meet with any trace of ordinary terrigenous sediment. Even the finest-grained portions appear under the microscope to consist of minute fragments of porous scoriaceous vitreous rock. There is no admixture of quartz-grains and argillaceous matter, such as might have been derived from the adjacent Silurian rocks. If such derivative matter occur at all it cannot be common, or I should hardly have missed it.

Intrusive Rocks.—Intrusive rocks, as already remarked, are well developed in the headland of St Abb's. These consist principally of a compact fine-grained blue porphyrite, which is often reddened with ferritic matter. It is a hard, tough rock, very irregularly jointed. Here and there it is sparingly porphyritic with "ferrite"—evidently pseudomorphous after pyroxene. The rock is a good deal weathered, and fresh specimens are not readily obtained. Under the microscope it shows a crypto-crystalline ground-mass—apparently composed entirely of small crystals of plagioclase, but owing to weathering the characteristic form and striation of the crystals are not well seen. There is no trace of non-individualised basis—nothing resembling the vitreous and devitrified basis of the bedded porphyrites. Scattered through this ground-mass occur occasional large crystals of plagioclase, and here and there more or less abundant pseudomorphs of limonite and serpentine after hornblende or pyroxene—the latter apparently being most common. Diallage, partially altered into limonite and serpentine, appears now and again; and probably many of the pseudomorphs just referred to may represent this mineral. Other decomposition-products diffused through the base in some sections are secondary quartz and chalcedony, and, in thin veins, calcite and hæmatite or limonite.*

(*b*) *Rocks of Coldingham Shore and Coldingham Bay.*

The igneous rocks of this area are separated from those of the region just described by a narrow belt of vertical Silurian strata which occupy the cliffs and foreshore between Halterem's Loup and the Long Carr. At Halterem's Loup the junction between the Old Red Sandstone and the Silurian is a well-marked fault. The greywackés are considerably hardened and shattered, more especially as they approach the igneous rocks of Coldingham Shore. At one or two places, near their junction with the latter, patches of tuff and agglomerate occur upon the foreshore, as if resting directly upon the Silurian; but these patches are evidently of an intrusive character, and appear to occupy vertical fisures in the surrounding greywackés, which are extremely hardened and altered and very much reddened. The actual junction of these greywackés with the main mass of the igneous rocks presently to be described is likewise strongly suggestive of the intrusive character of the latter. Similar appearances mark the junction between these rocks and the Silurian greywackés on the shores of Coldingham Bay. At that place the greywackés are much jumbled, broken, and hardened, and saturated with red ferritic matter. The Old Red igneous rocks do not overlie, but are intrusive in the Silurian strata.

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* At Bell Hill the conglomerate is traversed by a dyke of mica-trap or minette. This rock shows under the microscope a micro-crystalline ground-mass full of felspar microliths and black magnetite dust. Scattered abundantly through this ground-mass are small scales and larger crystals of biotite, many of which are broken and twisted, and contain inclusions of the ground-mass. Orthoclase appears sparingly, and a few irregular crystalline granules of quartz are present. Magnetite is very plentiful.

These igneous rocks consist partly of tuffs and agglomerates and partly of porphyrite. The fragmental rocks are, for the most part, quite unstratified, although here and there some trace of rude bedding may be noted. They are of a dull red colour, and are made up of angular and subangular fragments of all shapes and sizes up to blocks measuring over a yard in diameter. These are set in a matrix of smaller stones and comminuted débris. The whole appearance of this coarse tuff, which in many places is quite an agglomerate—that is to say, a coarse breccia of large stones and blocks—is similar to that of those tuffs and agglomerates which elsewhere in Scotland are found occupying the necks or throats of old volcanic orifices. All the fragments consist of varieties of porphyrite—a great many of which are precisely similar to the bedded porphyrites of St Abb's Head; others, however, are more markedly crystalline and porphyritic, especially with plagioclase felspar. A number of these fragments were examined under the microscope, but no new features of importance were disclosed. Highly amygdaloidal and scoriaceous fragments are common enough, but are not so strikingly prevalent as in the well-bedded tuffs of Horsecastle Bay, &c. One may say that, while in these latter scoriæ predominate and fragments of less porous rock do not abound, in the agglomerates of Coldingham Shore it is just the reverse. From the lapilli and blocks of this agglomerate, however, I did not succeed in getting any fresh specimens. Most of the rocks are much altered, so much so indeed that all one can determine is the fact that clouded crystals of plagioclase occur more or less numerously in a dull ferritic base. Patches and irregular aggregates of ferric oxide are common enough also, and probably represent hornblende or pyroxene. The less altered specimens often show a devitrified base, crowded with microliths and small crystals of plagioclase. Scattered through this ground-mass are many larger crystals of plagioclase. Upon the whole, highly porphyritic rocks appear to be of more frequent occurrence in this agglomerate than they are in the bedded series of porphyrites and tuffs at St Abb's Head.

The unstratified agglomerate is irregularly traversed by masses of close-grained porphyrite, which has the same general character as the intrusive porphyrite near St Abb's Head. This porphyrite is, for the most part, much reddened with iron oxides, and considerably weathered, so that fresh fractures are not readily obtained. Where least weathered, it is a somewhat compact blue or purplish rock. Here and there plagioclase felspar is conspicuous as a porphyritic ingredient, and now and again pyroxene (augite or diallage) or pseudomorphs after pyroxene are visible. Thin veins and threads of limonite and hæmatite are common.*

IV. GENERAL CONCLUSIONS AS TO THE OLD RED SANDSTONE SERIES.

The basement beds of this series, consisting of the conglomerate of Bell Hill, are a mere fragment, and we cannot say much about the conditions under which they were accumulated. They show us, what indeed may be learned in many other parts of Scotland, that the Lower Silurian strata had already been folded, crumpled, and contorted, and excessively denuded before Old Red Sandstone times. As the conglomerates contain not a single fragment of igneous rock, it is most probable that their formation preceded the outbreak of volcanic action in this neighbourhood.

The coarse tuff and agglomerate of Coldingham Shore fill up an old volcanic orifice, from which the porphyrites and porphyrite-tuffs of St Abb's had previously been ejected. The great intrusions of porphyrite which penetrate, not only the agglomerate of the "neck," but the bedded tuffs and porphyrites, evidently belong to a closing stage in the same period of volcanic activity. The dykes of Bell Hill and the harbour are certainly of much more recent date, and need not be considered in the present paper.

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* A dyke of basalt-rock crosses the intrusive porphyrites at the harbour, Coldingham Shore. Unfortunately, I neglected to bring away a specimen of the rock for closer examination. But I hope soon to revisit the district for further study, and to supply the omission in a subsequent paper to the Society.

The south-east dip of the bedded igneous rocks of St Abb's I take to be due to the two large faults which form their boundary lines. The original inclination of the strata, which need not have been great, would necessarily be away from the neck of Coldingham Shore. It is quite impossible to say what the extent of these faults may be, but it is probably considerable, as the following considerations will show. Neither the top nor the bottom of the bedded rocks is seen, but the actual thickness displayed is not less than 1200 feet. At present the beds dip towards S.E. at about 15°. If, therefore, we take the beds at Pettico Wick to represent the basement beds of the igneous series, which they certainly are not, then, to restore the beds to horizontality, it is evident that the tuffs at the south-east end of the headland of St Abb's would require to be lifted up for some 1200 feet. But, on the supposition that all these bedded rocks have been derived from the volcanic neck at Coldingham Shore, and would at first, therefore, have a dip towards the north, it is obvious that the subsequent downthrow produced by the N.W. and S.E. fault must considerably exceed 1200 feet—say, 1300 or 1400 feet. A glance at the section seen at Pettico Wick, however, shows us that the beds there are not the basement beds of the series, for they are faulted down against the Silurian. The actual base of the series is submerged. If, therefore, the present S.E. dip of the porphyrites and tuffs be due mainly, as I believe, to the fault at present referred to, the downthrow of that dislocation must increase from N.W. to S.E., until it reaches not less than 1300 feet. To the effect produced by this fault we have to add that of the fault at Halterem's Loup, which has a downthrow to N.W., and has therefore had its share in bringing about the S.E. dip of the bedded porphyrites and tuffs. It is impossible, however, to form even an approximate estimate of the amount of downthrow produced by this latter fault. From the fact that it seems to cut off the other, no trace of which occurs in the rocks to the south, we may infer that its downthrow is considerable. Be that as it may, it is quite certain that the tuffs of Horsecastle Bay occupy a much higher horizon than the agglomerates exposed upon the beach at Coldingham Shore.

While the geological structure of St Abb's Head thus leads to the conclusion that the bedded tuffs and porphyrites formerly dipped in a northerly direction, and may thus have been ejected from the volcanic focus in their neighbourhood, the petrological evidence lends additional support to this conclusion. Fragments of precisely the same porphyrites as those of St Abb's Head occur abundantly in the rock at Coldingham Shore. A few blocks and fragments of greywacké were observed in the agglomerate, and may, perhaps, be more plentiful than they seem to be, for, of course, the agglomerate is only partially exposed; but with these exceptions all the stones I saw were porphyrites. Sections of the finer-grained portions of the tuff examined under the microscope showed in like manner that these are composed of the comminuted débris of porphyrites.

From the fact that the bedded porphyrite-tuffs of St Abb's Head have evidently been arranged by and accumulated under water, we may infer that the whole series, so far as that is exposed, is of subaqueous origin—that, in the rocks now described, we have the relics of an old subaqueous volcano of Old Red Sandstone times. The bedded porphyrites offer many analogies with those of the Cheviot Hills, the Sidlaws, and other regions of Old Red Sandstone volcanic rocks. They are, in fact, only altered andesites, which, in their microscopic structure, reproduce exactly what one sees in the andesites of the Western Territories of North America. The intrusive porphyrites, however, which intersect the agglomerate and the bedded rocks hardly resemble the intrusive masses of the Sidlaws, the Cheviots, &c., those of the Sidlaws being mainly diabase (altered basalt-rock), while those of the Cheviots are chiefly granite and felsite. Again, the bedded scoriæ-tuffs of St Abb's Head are hardly paralleled by any tuffs met with either in the Cheviots, the Sidlaws, or any other region of Old Red Sandstone volcanic rocks known to me. Of course, lapilli of highly porous and amygdaloidal porphyrites occur commonly enough in many of those districts, but nowhere, so far as I have seen, have we such a depth of fragmental materials consisting almost exclusively of scoriæ or cinders. These form the highest beds of the series, and possibly represent the latest ejections from the adjoining volcanic orifice. But they have suffered great denudation, and certainly attained a greater thickness, and overspread a much wider area than they now occupy. This is evident from the appearance at the surface of the intrusive porphyrites near St Abb's Head. These, there can be little doubt, cooled and consolidated under the surface—the tuff which formerly covered them has been washed away. The intrusion of these porphyrites marks the closing stage in the volcanic activity of Old Red Sandstone times. It was not until after lavas and fragmental materials had ceased to be erupted, and the throat of the old volcano had become plugged with angular débris and blocks, that the rocks in question were injected. They evidently cooled under some pressure, but were probably not of very deep-seated origin. Whether they ever rose to the actual surface of the old volcano and overflowed, we cannot tell, for the whole of that surface has of course disappeared. It is obvious, however, that the molten masses from which they came must have been of essentially the same composition as that from which the bedded porphyrites were derived.

V. Glacial Phenomena.

The district described in this paper has been entirely overflowed by ice. This is proved by the generally glaciated contour of the rocks, which are smoothed from N.W. to S.E. The escarpments of porphyrites that faced the ice-flow have been bevelled off, and, notwithstanding the weathered character of the rocks, well-marked striæ are met with here and there, the trend of which is E. 35 S. Glaciated stones and clay occur in patches in the hollows of the headland, and a considerable mass of till occupies the hollow that separates the headland from the Silurian uplands. This is well exposed in section at Pettico Wick. It presents no features that require to be noted here, but is made up of the débris of rocks which have come from the west or north-west.

PRIVATE BUSINESS.

A ballot was taken, and the following gentlemen were duly elected Fellows of the Society:—Mr Alexander Goodman More, Alexander M. M'Aldowie, M.D., Mr James Hunter, Mr William Gilmour, Mr Herbert H. Ashdown, M.B., and Mr J. Mackay Bernard.

Monday, 18th April 1887.

SIR WILLIAM THOMSON, President, in the Chair.

Professor Rowland's Photographs of the Solar Spectrum were exhibited by the Astronomer-Royal for Scotland.

The following Communications were read:—

1. On Ship-Waves. By Sir W. Thomson.

2. On the Instability in Fluid Motion. By the Same.

3. Experimental Research in Magnetism. By Mr D. S. Sinclair. Communicated by Principal Jamieson.

4. On the Summation of certain Series of Alternants.

By A. H. Anglin, M.A., LL.B., &c.

1. The summations referred to in the title depend on two theorems—one already known, the other not hitherto published. The first of these is to the effect that *the quotient of any simple Alternant by the difference-product of the variables is expressible as a determinant whose elements are sums of homogeneous products of the variables.* For example,

$$\begin{vmatrix} 1 & a^p & a^q & a^r \\ 1 & b^p & b^q & b^r \\ 1 & c^p & c^q & c^r \\ 1 & d^p & d^q & d^r \end{vmatrix} = \zeta^{\frac{1}{2}}(abcd) \begin{vmatrix} (p-1) & (p-2) & (p-3) \\ (q-1) & (q-2) & (q-3) \\ (r-1) & (r-2) & (r-3) \end{vmatrix},$$

where, generally, (n) denotes the sum of the homogeneous products of a, b, c, d of n dimensions.

The other theorem may be enunciated, in a particular instance, as follows:—

If three rows of four quantities each be taken, as

$$\begin{matrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 & c_4 \end{matrix},$$

and every possible determinant of the third degree be formed from this matrix by deleting the last column of a row and the first element of the other rows, the sum of these determinants is equal to the determinant got from the matrix by deleting the second column: thus

$$\begin{vmatrix} a_1 & a_2 & a_3 \\ b_2 & b_3 & b_4 \\ c_2 & c_3 & c_4 \end{vmatrix} + \begin{vmatrix} a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 \\ c_2 & c_3 & c_4 \end{vmatrix} + \begin{vmatrix} a_2 & a_3 & a_4 \\ b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 \end{vmatrix} = \begin{vmatrix} a_1 & a_3 & a_4 \\ b_1 & b_3 & b_4 \\ c_1 & c_3 & c_4 \end{vmatrix}$$

and a like series equal to the determinant got by deleting the third column, namely

$$\begin{vmatrix} a_1 & a_2 & a_3 \\ b_1 & b_2 & b_3 \\ c_2 & c_3 & c_4 \end{vmatrix} + \begin{vmatrix} a_1 & a_2 & a_3 \\ b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 \end{vmatrix} + \begin{vmatrix} a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 \\ c_1 & c_2 & c_3 \end{vmatrix} = \begin{vmatrix} a_1 & a_2 & a_4 \\ b_1 & b_2 & b_4 \\ c_1 & c_2 & c_4 \end{vmatrix}.$$

2. The proof of any case of this latter theorem depends on the case before it. Thus, taking the matrix

$$\begin{matrix} a_1 & a_2 & a_3 \\ b_1 & b_2 & b_3 \end{matrix},$$

the identity for the case of determinants of the second order is

$$\begin{vmatrix} a_1 & a_2 \\ b_2 & b_3 \end{vmatrix} + \begin{vmatrix} a_2 & a_3 \\ b_1 & b_2 \end{vmatrix} = \begin{vmatrix} a_1 & a_3 \\ b_1 & b_3 \end{vmatrix} = |a_1 \; b_3| \quad . \quad . \quad (1),$$

the truth of it being self-evident.

Turning to the case for determinants of the third order, and expanding each determinant in the left-hand side in terms of the elements of the first column, we see that the coefficient of a_1 is $|b_3 \; c_4|$, while the coefficient of a_2 consists of the sum of two determinants which by (1) is equal to $|b_2 \; c_4|$; and likewise for the coefficients of the elements involving b's and c's. Hence we get

$$\begin{vmatrix} a_1 & a_3 & a_4 \\ b_1 & b_3 & b_4 \\ c_1 & c_3 & c_4 \end{vmatrix} + \begin{vmatrix} a_2 & a_2 & a_4 \\ b_2 & b_2 & b_4 \\ c_2 & c_2 & c_4 \end{vmatrix};$$

and thus

$$\Sigma \begin{vmatrix} a_1 & a_2 & a_3 \\ b_2 & b_3 & b_4 \\ c_2 & c_3 & c_4 \end{vmatrix} = \begin{vmatrix} a_1 & a_3 & a_4 \\ b_1 & b_3 & b_4 \\ c_1 & c_3 & c_4 \end{vmatrix} = |a_1 \; b_5 \; c_4| \quad . \quad . \quad (2);$$

and in like manner,

$$\Sigma \begin{vmatrix} a_1 & a_2 & a_3 \\ b_1 & b_2 & b_3 \\ c_2 & c_3 & c_4 \end{vmatrix} = \begin{vmatrix} a_1 & a_2 & a_4 \\ b_1 & b_2 & b_4 \\ c_1 & c_2 & c_4 \end{vmatrix} = | a_1 \; b_2 \; c_4 | \quad . \quad . \quad (3).$$

3. Taking the next case, arising out of the matrix

$$\begin{matrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5 \\ c_1 & c_2 & c_3 & c_4 & c_5 \\ d_1 & d_2 & d_3 & d_4 & d_5 , \end{matrix}$$

consisting of four rows of five quantities each: we have to find the value of

$$\Sigma \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_2 & b_3 & b_4 & b_5 \\ c_2 & c_3 & c_4 & c_5 \\ d_2 & d_3 & d_4 & d_5 \end{vmatrix} \text{ or } \Sigma_1 ,$$

where the suffix-order (1 2 3 4) occurs once in each determinant and Σ_1 consequently consists of four terms. Expanding the determinants as before, we see that the coefficient of a_1 is $| b_3 \; c_4 \; d_5 |$, while the coefficient of a_2 consists of three determinants whose sum by (2) is equal to $| b_2 \; c_4 \; d_5 |$; and similarly for the coefficients of the elements involving b, c, d. Hence we get

$$\begin{vmatrix} a_1 & a_3 & a_4 & a_5 \\ b_1 & b_3 & b_4 & b_5 \\ c_1 & c_3 & c_4 & c_5 \\ d_1 & d_3 & d_4 & d_5 \end{vmatrix} + \begin{vmatrix} a_2 & a_2 & a_4 & a_5 \\ b_2 & b_2 & b_4 & b_5 \\ c_2 & c_2 & c_4 & c_5 \\ d_2 & d_2 & d_4 & d_5 \end{vmatrix} ;$$

and thus

$$\Sigma_1 = | a_1 \; b_3 \; c_4 \; d_5 | \quad . \quad . \quad . \quad . \quad . \quad (4).$$

Again, to find the value of

$$\Sigma \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \\ c_2 & c_3 & c_4 & c_5 \\ d_2 & d_3 & d_4 & d_5 \end{vmatrix} \text{ or } \Sigma_2 ,$$

consisting of six terms, in each of which the suffix-order (1 2 3 4) occurs twice. Expanding in the same manner as before, it will be seen that the coefficient of a_1 consists of the sum of three determinants which by (2) is equal to $| b_2 \; c_4 \; d_5 |$, while the coefficient of a_2 likewise involves three determinants whose sum by (3) is equal to $| b_2 \; c_3 \; d_5 |$. Similar expressions holding for the coefficients of the other elements in the first columns, we get

$$| a_1 \; b_2 \; c_4 \; d_5 | + | a_2 \; b_2 \; c_3 \; d_5 | ;$$

and thus

$$\Sigma_2 = | a_1 \; b_2 \; c_4 \; d_5 | \quad . \; . \; . \; . \; . \; . \quad (5).$$

Lastly, it may be shown in like manner that, in the case of

$$\Sigma \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 & c_4 \\ d_2 & d_3 & d_4 & d_5 \end{vmatrix} \text{ or } \Sigma_3 ,$$

consisting of four terms, in each of which the suffix-order (1 2 3 4) occurs three times,

$$\Sigma_3 = | a_1 \; b_2 \; c_3 \; d_5 | \quad . \; . \; . \; . \; . \; . \quad (6).$$

The results (4), (5), and (6) constitute the theorem in the case of determinants of the fourth order, and are formed by deleting successively the second, third, and fourth columns from the matrix out of which they arise.

4. Thus, assuming the truth of the identities for the case of determinants of the $(n-1)$th order, we can establish the corresponding results for the case of determinants of the nth order; that is to say, taking the matrix

$$\begin{matrix} a_1 & a_2 & a_3 & \dots & a_{n+1} \\ b_1 & b_2 & b_3 & \dots & b_{n+1} \\ c_1 & c_2 & c_3 & \dots & c_{n+1} \\ \cdot & \cdot & \cdot & & \cdot \\ \cdot & \cdot & \cdot & & \cdot \\ l_1 & l_2 & l_3 & \dots & l_{n+1} , \end{matrix}$$

consisting of n rows of $n+1$ quantities each, we can obtain the following $n-1$ results involving determinants of the nth order, and which are formed by deleting successively the second, third, fourth, . . . , nth columns from the matrix, viz.:—

$$\Sigma\begin{vmatrix} a_1 & a_2 & \dots & a_n \\ b_2 & b_3 & \dots & b_{n+1} \\ c_2 & c_3 & \dots & c_{n+1} \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ l_2 & l_3 & \dots & l_{n+1} \end{vmatrix} \text{ or } \Sigma_1 \quad = | a_1 \; b_3 \; c_4 \dots l_{n+1} |$$

$$\Sigma\begin{vmatrix} a_1 & a_2 & \dots & a_n \\ b_1 & b_2 & \dots & b_n \\ c_2 & c_3 & \dots & c_{n+1} \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ l_2 & l_3 & \dots & l_{n+1} \end{vmatrix} \text{ or } \Sigma_2 \quad = | a_1 \; b_2 \; c_4 \dots l_{n+1} |$$

$$\Sigma\begin{vmatrix} a_1 & a_2 & \dots & a_n \\ b_1 & b_2 & \dots & b_n \\ c_1 & c_2 & \dots & c_n \\ d_2 & d_3 & \dots & d_{n+1} \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ l_2 & l_3 & \dots & l_{n+1} \end{vmatrix} \text{ or } \Sigma_3 \quad = | a_1 \; b_2 \; c_3 \; d_5 \dots l_{n+1} |$$

. . . .

$$\Sigma\begin{vmatrix} a_1 & a_2 & \dots & a_n \\ b_1 & b_2 & \dots & b_n \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ k_1 & k_2 & \dots & k_n \\ l_2 & l_3 & \dots & l_{n+1} \end{vmatrix} \text{ or } \Sigma_{n-1} = | a_1 \; b_2 \; c_3 \dots k_{n-1} \; l_{n+1} | ,$$

where, generally, Σ_μ consists of ${}_nC_\mu$ determinants in each of which the suffix-order $(1\ 2\ 3 \dots n)$ occurs μ times,—${}_nC_\mu$ denoting the number of combinations of n things taken μ together.

We now propose, with the help of these identities, to effect the summation of certain series of Alternants, obtaining results which may be called *Extensions* of the known theorem referred to at the outset.

1. Employing n letters $a, b, c, \ldots h, k, l$—starting with the case of two general indices y and z, we have

$$|a^0b^1c^2 \ldots h^{n-3}k^yl^z| = \zeta^{\frac{1}{2}}(abc \ldots l)\,|(y-n+2),\ (z-n+1)|,$$

which, for shortness, may be written in the form

$$[y, z] = |2, 1|\,\zeta^{\frac{1}{2}} \quad \ldots\ldots \quad \text{(A)}.$$

Then

$$[y+1, z] + [y, z+1] = \begin{vmatrix} 3 & 2 \\ 2 & 1 \end{vmatrix} \zeta^{\frac{1}{2}} + \begin{vmatrix} 2 & 1 \\ 3 & 2 \end{vmatrix} \zeta^{\frac{1}{2}}$$

$$= \begin{vmatrix} 3 & 1 \\ 3 & 1 \end{vmatrix} \zeta^{\frac{1}{2}} = |3, 1|\,\zeta^{\frac{1}{2}} \quad \ldots \quad (1),$$

which result is the only Extension of equation (A).

Again, in the case of three general indices x, y, z, writing the equation

$$|a^0b^1c^2 \ldots g^{n-4}h^xk^yl^z| = \zeta^{\frac{1}{2}}(abc \ldots l)\,|(x-n+3), (y-n+2), (z-n+1)|$$

in the form

$$[x, y, z] = |3, 2, 1|\,\zeta^{\frac{1}{2}} \quad \ldots\ldots \quad \text{(B)},$$

we have

$$[x+1, y, z] + [x, y+1, z] + [x, y, z+1] = S_1\zeta^{\frac{1}{2}},$$

where

$$S_1 = \begin{vmatrix} 4 & 3 & 2 \\ 3 & 2 & 1 \\ 3 & 2 & 1 \end{vmatrix} + \begin{vmatrix} 3 & 2 & 1 \\ 4 & 3 & 2 \\ 3 & 2 & 1 \end{vmatrix} + \begin{vmatrix} 3 & 2 & 1 \\ 3 & 2 & 1 \\ 4 & 3 & 2 \end{vmatrix}.$$

Expanding each determinant in terms of the elements of the first column, the coefficient of $x-n+4$ is $|2, 1|$, while that of $x-n+3$ consists of the sum of two determinants which by (1) is equal to $|3, 1|$; and similarly for the coefficients of the elements involving the other indices.

Hence

$$S_1 = \begin{vmatrix} 4 & 2 & 1 \\ 4 & 2 & 1 \\ 4 & 2 & 1 \end{vmatrix} + \begin{vmatrix} 3 & 3 & 1 \\ 3 & 3 & 1 \\ 3 & 3 & 1 \end{vmatrix},$$

and thus

$$\Sigma[x+1, y, z] = |4, 2, 1|\,\zeta^{\frac{1}{2}} \quad \ldots\ldots \quad (2).$$

Further, we have

$$[x+1, y+1, z] + [x+1, y, z+1] + [x, y+1, z+1] = S_2\zeta^{\frac{1}{2}},$$

where

$$S_2 = \begin{vmatrix} 4 & 3 & 2 \\ 4 & 3 & 2 \\ 3 & 2 & 1 \end{vmatrix} + \begin{vmatrix} 4 & 3 & 2 \\ 3 & 2 & 1 \\ 4 & 3 & 2 \end{vmatrix} + \begin{vmatrix} 3 & 2 & 1 \\ 4 & 3 & 2 \\ 4 & 3 & 2 \end{vmatrix},$$

which, in the same manner as before, may be shown to be equal to

$$\begin{vmatrix} 4 & 3 & 1 \\ 4 & 3 & 1 \\ 4 & 3 & 1 \end{vmatrix} + \begin{vmatrix} 3 & 3 & 2 \\ 3 & 3 & 2 \\ 3 & 3 & 2 \end{vmatrix},$$

and thus

$$\Sigma[x+1,\ y+1,\ z] = |4,\ 3,\ 1|\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad (3).$$

These two results are the Extensions of equation (B), and are formed by deleting successively the second and third figures from the series 4, 3, 2, 1; that is to say, the second and third columns from the matrix

$$\begin{matrix} x-n+4, & x-n+3, & x-n+2, & x-n+1 \\ y-n+4, & y-n+3, & y-n+2, & y-n+1 \\ z-n+4, & z-n+3, & z-n+2, & z-n+1. \end{matrix}$$

2. Again, in the case of four general indices u, x, y, z, writing the equation

$$|a^0b^1c^2 \ldots f^{n-5}g^uh^xk^yl^z| = |(u-n+4), (x-n+3), y-n+2), (z-n+1)|\zeta^{\frac{1}{2}}$$

in the form

$$[u,\ x,\ y,\ z] = |4,\ 3,\ 2,\ 1|\zeta^{\frac{1}{2}}, \quad . \quad . \quad . \quad . \quad (C),$$

we have

$$\Sigma[u+1,\ x,\ y,\ z] = S_1\zeta^{\frac{1}{2}},$$

Σ consisting of four terms in each of which there is one index of the form $\lambda+1$, and where

$$S_1 = \begin{vmatrix} 5 & 4 & 3 & 2 \\ 4 & 3 & 2 & 1 \\ 4 & 3 & 2 & 1 \\ 4 & 3 & 2 & 1 \end{vmatrix} + \begin{vmatrix} 4 & 3 & 2 & 1 \\ 5 & 4 & 3 & 2 \\ 4 & 3 & 2 & 1 \\ 4 & 3 & 2 & 1 \end{vmatrix} + \begin{vmatrix} 4 & 3 & 2 & 1 \\ 4 & 3 & 2 & 1 \\ 5 & 4 & 3 & 2 \\ 4 & 3 & 2 & 1 \end{vmatrix} + \begin{vmatrix} 4 & 3 & 2 & 1 \\ 4 & 3 & 2 & 1 \\ 4 & 3 & 2 & 1 \\ 5 & 4 & 3 & 2 \end{vmatrix}.$$

Expanding these determinants as before, it may be shown by the application of equation (2) that

$$S_1 = |5,\ 3,\ 2,\ 1| + |4,\ 4,\ 2,\ 1|;$$

and we thus have

$$\Sigma[u+1,\ x,\ y,\ z] = |5,\ 3,\ 2,\ 1|\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad (4).$$

Further, we have

$$\Sigma[u+1,\ x+1,\ y,\ z] = S_2\zeta^{\frac{1}{2}} \text{ suppose,}$$

where Σ consists of six terms in each of which there are two indices of the form $\lambda+1$, and where by equations (2) and (3) it will be found that

$$S_2 = |5,\ 4,\ 2,\ 1| + |4,\ 4,\ 3,\ 1|\,;$$

and consequently

$$\Sigma[u+1,\ x+1,\ y,\ z] = |5,\ 4,\ 2,\ 1\ \ \zeta^{\frac{1}{2}}\ \ .\ \ .\ \ .\quad (5).$$

Lastly, it may be shown in like manner that

$$\begin{aligned}\Sigma[u+1,\ x+1,\ y+1,\ z] &= (|5,\ 4,\ 3,\ 1| + |4,\ 4,\ 3,\ 2|)\,\zeta^{\frac{1}{2}}\\ &= |5,\ 4,\ 3,\ 1|\,\zeta^{\frac{1}{2}}\ \ .\ \ .\ \ .\ \ .\ \ .\quad (6).\end{aligned}$$

The results (4), (5), and (6) are the Extensions of equation (C), and their right-hand members are formed by deleting successively the second, third, and fourth figures from the series 5, 4, 3, 2, 1; that is, by deleting these columns successively from the corresponding matrix. We may further observe that if we increase by unity *one* index in the left-hand side of (C), the sum of the resulting alternants is obtained by increasing by unity the elements of *one* column in the right-hand side, thus giving the identity (4); while an increase in two and three indices produces respectively a corresponding increase in the elements of two and three columns, thus furnishing equations (5) and (6).

3. To obtain the Extensions involving any number (ν) of indices we should thus assume the corresponding results for $\nu-1$ indices, and then deduce those for ν.

Now, in the case of $n-3$ general indices $r,\ s,\ t,\ \ldots\ z$, we have

$$|a^0b^1c^2d^re^s\ \ldots\ l^z| = |(r-3),\ (s-4),\ \ldots,\ (z-n+1)|\,\zeta^{\frac{1}{2}}\,,$$

which may be written in the form

$$[r,\ s,\ t,\ \ldots\ z] = |3,\ 4,\ 5,\ \ldots\ n=1|\,\zeta^{\frac{1}{2}},$$

the Extensions of which are

$$\begin{aligned}\Sigma[r+1,\ s,\ t,\ \ldots\ z] &= |2,\ 4,\ 5,\ \ldots\ n-1|\,\zeta^{\frac{1}{2}}\ \ .\ \ .\ \ .\ \ .\quad (1)'\\ \Sigma[r+1,\ s+1,\ t,\ \ldots\ z] &= |2,\ 3,\ 5,\ \ldots\ n-1|\,\zeta^{\frac{1}{2}}\ \ .\ \ .\ \ .\quad (2)'\\ \Sigma[r+1,\ s+1,\ t+1,\ u,\ \ldots\ z] &= |2,\ 3,\ 4,\ 6,\ \ldots\ n-1|\,\zeta^{\frac{1}{2}}\quad (3)'\\ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\end{aligned}$$

while generally

$$\Sigma_\mu = |2,\ 3,\ 4,\ \ldots\ \mu+1,\ \mu+3,\ \ldots\ n-1|\,\zeta^{\frac{1}{2}}\quad .\ \ (\mu)'$$

and lastly,

$$\Sigma[r+1,\ s+1,\ \ldots\ y+1,\ z] = |2,\ 3,\ 4,\ \ldots\ n-3,\ n-1|\,\zeta^{\frac{1}{2}}\ldots(n-4)'$$

where, in general, Σ_μ consists of ${}_{n-3}C_\mu$ terms in each of which there are μ indices of the form $\lambda+1$.

To deduce the Extensions involving $n-2$ general indices $q, r, s, \ldots z$ from these $n-4$ results,—we have

$$|a^0b^1c^qd^r \ldots l^z| = |(q-2), (r-3), \ldots, (z-n+1)|\zeta^{\frac{1}{2}},$$

that is, in the above notation,

$$[q, r, s, \ldots z] = |2, 3, 4, \ldots n-1|\zeta^{\frac{1}{2}} \quad . \quad . \quad (D).$$

Then, if

$$\Sigma[q+1, r, s, \ldots z] \text{ or } \Sigma_1 = S_1\zeta^{\frac{1}{2}},$$

on writing down the determinants in S_1 and expanding them in terms of the elements of the first column in each, it may be shown by equation $(1)'$ that

$$S_1 = |1, 3, 4, \ldots n-1| + |2, 2, 4, 5, \ldots n-1|,$$

and thus

$$\Sigma_1 = |1, 3, 4, \ldots n-1|\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad (1).$$

Again, if

$$\Sigma[q+1, r+1, s, \ldots z] \text{ or } \Sigma_2 = S_2\zeta^{\frac{1}{2}},$$

we shall find in like manner by equations $(1)'$ and $(2)'$ that

$$S_2 = |1, 2, 4, 5, \ldots n-1| + |2, 2, 3, 5, \ldots n-1|,$$

and consequently

$$\Sigma_2 = |1, 2, 4, 5, \ldots n-1|\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad (2),$$

while by the application of $(2)'$ and $(3)'$ it may further be shown that

$$\Sigma[q+1, r+1, s+1, t, \ldots z] \text{ or } \Sigma_3 = |1, 2, 3, 5, \ldots n-1|\zeta^{\frac{1}{2}} \,.\, (3).$$

Generally, to find the corresponding value of Σ_μ, having ${}_{n-2}C_\mu$ terms in each of which there are μ indices of the form $\lambda+1$,—suppose

$$\Sigma_\mu = S_\mu\zeta^{\frac{1}{2}}.$$

Now since in Σ_μ there are ${}_{n-3}C_{\mu-1}$ terms with the index $q+1$, and ${}_{n-3}C_\mu$ terms with the index q, on writing down and expanding the determinants in S_μ as before, it will be seen that the coefficients of $q-1$ consists of the sum of ${}_{n-3}C_{\mu-1}$ determinants which by equation $(\mu-1)'$ is equal to $|2, 3, 4, \ldots \mu, \mu+2, \ldots n-1|$, while the coefficient of $q-2$ consists of the sum of ${}_{n-3}C_\mu$ determinants which by equation $(\mu)'$ is equal to $|2, 3, 4, \ldots \mu+1, \mu+3, \ldots n-1|$; and likewise for the coefficients of the elements involving the indices $r, s, t, \dots z$. Hence we have

$$\begin{aligned} S_\mu = & \,|\,1,\ 2,\ 3,\ \dots \mu,\ \mu+2,\ \dots n-1\,| \\ & + |\,2,\ 2,\ 3,\ \dots \mu+1,\ \mu+3,\ \dots n-1\,|, \end{aligned}$$

and thus

$$\Sigma_\mu = |\,1,\ 2,\ 3,\ \dots \mu,\ \mu+2,\ \dots n-1\,|\,\zeta^{\frac{1}{2}} \quad . \quad . \quad (\mu).$$

Lastly, the corresponding value of

$$\Sigma[q+1,\ r+1,\ \dots y+1,\ z] \text{ or } \Sigma_{n-3}$$

is deduced in like manner by the use of equation $(n-4)'$, when we get

$$\Sigma_{n-3} = |\,1,\ 2,\ 3,\ \dots n-3,\ n-1\,|\,\zeta^{\frac{1}{2}} \quad . \quad . \quad (n-3).$$

The results (1), (2), (3), . . . $(n-3)$ are the Extensions of equation (D), and their right-hand members are formed by deleting successively the second, third, fourth, . . ., $(n-2)$th figures from the series $1, 2, 3, \dots n-2, n-1$; that is, by deleting these columns successively from the corresponding matrix. And we may further notice that the coefficients of $\zeta^{\frac{1}{2}}$ in these $n-3$ identities (corresponding respectively to an increase by unity in $1, 2, 3, \dots n-3$ indices), are also formed by increasing by unity the elements of $1, 2, 3, \dots n-3$ columns respectively of the determinant $|\,(q-2),\ (r-3),\ (s-4),\ \dots,\ (z-n+1)\,|$.

5. Note on Cobaltic Alums. By Mr Hugh Marshall, B.Sc.

From a peculiar change of colour observed in the electrolysis of a solution of copper-cobalt potassium sulphate, the author was recently induced to make some experiments on the behaviour of cobalt sulphate solution when electrolysed in such a manner that the reducing product of the decomposition could not act on the solution. For this purpose a divided cell was used, so that the two electrodes were practically in separate vessels. The apparatus will be fully described in a future communication.

It was found that an acid solution of cobalt sulphate alone is not changed when submitted to electrolysis in the apparatus. If, however, ammonium or potassium sulphate be also present, the solution passes through a series of changes of colour, ultimately becoming greenish-blue, and this, it was found, is due to oxidation of the cobalt from the cobaltous to the cobaltic state. This seemed to point to the probable formation of an alum. Experiments were therefore made in order to obtain crystals. The action was allowed to go on for several days, so as to concentrate the liquid; when, in the solution with ammonium sulphate, dark blue crystals separated out. These were octahedral, showing also faces of the cube and rhombic dodecahedron. The analysis of these showed that they were ammonium-cobalt alum.

		Theory.
Co_2O_3	17·87	17·05
$(NH_4)_2O$	5·18	5·36
SO_3	32·81	33·02
H_2O (By Diff.)	44·14	44·57
	100·00	100·00

When thoroughly dried the crystals are not unstable, but when dissolved in water are quickly reduced. The solution in sulphuric acid is not so soon decomposed. When the acid has been completely removed from the crystals, water seems to resolve them first into the constituent sulphates. The oxygen liberated during decomposition is partially in the form of ozone.

No satisfactory result was at first obtained with the potassium sulphate solution, chiefly owing to the slight solubility of that salt. If sufficient be not added the alum is decomposed. If, on the other hand, too much be added, it crystallises out. The happy medium can only be obtained by the addition of one or other salt. After several experiments, crystals were obtained similar to those of the ammonium alum, but they could not be separated from the potassium sulphate with which they were mixed. Afterwards a finely-crystalline deposit was obtained, which on analysis appeared to be the alum mixed with potassic sulphate.

Under similar conditions, nickel sulphate undergoes no change whatever.

Other salts of cobalt undergo oxidation in presence of corresponding alkali salts, forming evidently double salts; these are at present undergoing examination.

6. On the Effect produced on the Polarisation of Nerve by Stimulation. By Mr G. N. Stewart.

In this short paper I wish merely to give a summary of the chief results of experiments made by me in February and March last in the Physiological Institute of Berlin. Du Bois Reymond has recently made an elaborate investigation of the galvanic polarisation of muscle. Many years ago he made similar observations on nerve. The question which occurred to me was, How does tetanus affect the amount of polarisation in nerve? Apparently there has been no previous work at this subject. It is not necessary to describe here the arrangements used, nor the manner of making the observations, as a detailed account, with discussion of the facts brought out, will appear in the *Journal of Physiology.* When a current is passed through a living nerve, the polarisation produced may be either negative or positive. When it is negative, the polarisation current is, of course, in the opposite direction to the polarising current. Positive polarisation gives a current in the same direction as the polarising stream. It depends upon the " density " of the polarising current, and upon the time of flow, whether the deflection due to the polarisation is purely negative, purely positive, or of double sign. When it is of double sign, there is first a negative kick, which is followed by a more persistent positive deflection. As a matter of fact, there is every ground for believing, that in general the two polarisations exist side by side in the polarised nerve.

The influence of stimulation on the polarisation, so far as my experiments go, may be stated thus:—

1. *In every case the effect produced by stimulation is in the direction of diminution of the positive polarisation.*—One says "in the direction of," because increase of negative polarisation would equally well explain the result. It is probable, however, for reasons which need not be given here that it is the positive polarisation which is affected.

2. *The effect is, within limits, greater the longer the time of flow of the polarising stream.*—This has only been shown for weak currents, because strong currents depress the vitality of the nerve when they are allowed to flow for any length of time. If, in the case of a weak

polarising stream, a curve be constructed whose ordinates correspond to amount of change of the polarisation, the abscissæ representing time of flow, this curve is found to have a maximum; *i.e.*, when the current is kept constant, the effect first increases with the time, and then diminishes. Here the stimulus is, of course, also kept constant.

3. *The effect increases, within limits, with the density of the polarising stream.*—The curve plotted with current density as abscissa, and amount of stimulation effect as ordinate, also reaches a maximum, when the time of flow is kept constant.

4. *The effect is less, as might be expected, the longer the interval between breaking the polarising current and closing the galvanometer circuit.*—The persistency of it is sometimes, however, astonishing.

5. *The effect increases with the strength of the stimulus.*—Result No. 1 has an important bearing on Hermann's explanation of the apparent diminution of resistance in a tetanised nerve, which I propose to discuss at another time.

I must add that, in the case of a strong current, result No. 4 does not always hold, the nerve apparently requiring some time to regain its maximum of excitability.